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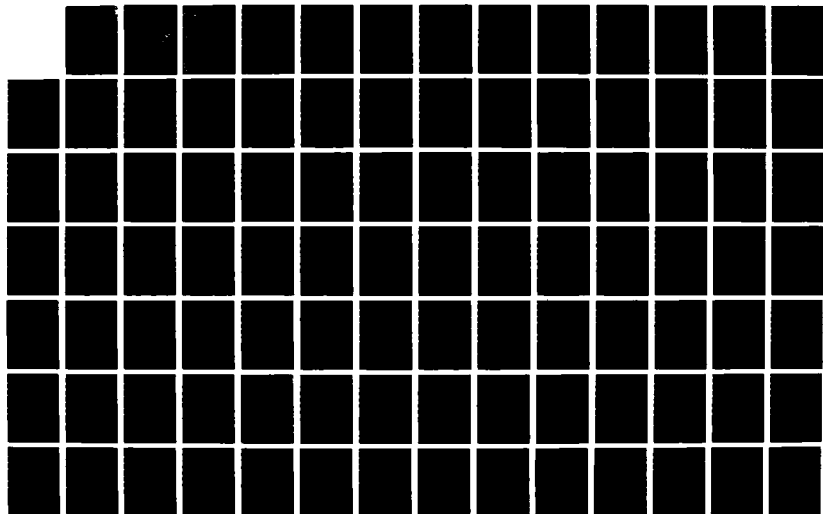
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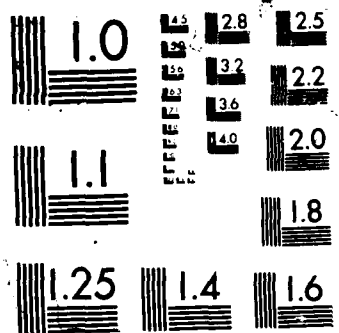
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**PROCEEDINGS OF THE
FORTIETH ANNIVERSARY SYMPOSIUM
OF THE
JOINT SERVICES ELECTRONICS PROGRAM (JSEP)**

**David Robb and
Arnold Shostak, Editors
ANSER**

FOR

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Preface

These proceedings are those of a symposium held on 25 September 1986 at the National Academy of Sciences, Washington, D.C., to commemorate and celebrate the establishment, in 1946, of the U.S. Joint Services Electronics Program (JSEP). In that year, forward-looking scientists and administration leaders at universities that had been engaged in wartime-related research, and military service agencies of the federal government, established JSEP for the purpose of carrying on university-type research of interest to all components of the military. The first of the universities involved were the Massachusetts Institute of Technology, Columbia University, Harvard University, and the Polytechnic Institute of Brooklyn. These were soon followed by Stanford University, the University of California, Berkeley, the Universities of Illinois, Southern California, Texas, and others. There are presently 12 universities in the program, which has expanded and contracted from time to time as interest and available funds have changed.

By any measure, JSEP has been a success. Results of investigations in the program have included atomic clocks, masers, lasers, much in communications theory and practice, as well as in microelectronics and high-speed electronic digital computation and circuitry. These results have found immense practical application in U.S. military operations.

The reader will find these proceedings valuable as an historic recording of events leading to initiation and successful pursuit of a great national asset--the U.S. Joint Services Electronics Program.

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OPENING REMARKS

Dr. Leo Young
Office of the Under Secretary of Defense
(Research and Engineering)

It is with genuine pleasure that I open the 40th Anniversary Symposium of the Joint Services Electronics Program--JSEP, as it is known affectionately--and to welcome you here: Secretary Taft, our own elder statesman of electronics, the old-timers who reminisced so movingly at last night's dinner, distinguished speakers, Nobel laureates, prize winners, honored guests.

During my 5 years as Director for Research and Laboratory Management, and for many years before that, I was impressed with the high quality and continuing productivity of JSEP research. It has given me the opportunity to meet with outstanding scientists and engineers at JSEP reviews, and I have particularly enjoyed meeting with the JSEP directors at least annually.

Today, as we look back on 40 highly successful years of JSEP operations and contributions, we may view JSEP in a broader context of cooperation, not only between the Department of Defense and academia, but also with important industry interactions. I believe that this cooperation is the key to our future, and we look forward to many more years of pioneering JSEP accomplishments.

It is both an honor and a pleasure for me to introduce our keynote speaker, the Honorable Deputy Secretary of Defense, William Howard Taft, IV. Secretary Taft has followed closely the R&D programs in Defense, and has supported them strongly, including basic research, of which JSEP is a part. He was sworn into his present office in February 1984, previously having served as DOD General Counsel since April 1981. He has worked for the Federal Government

in the Federal Trade Commission, the Office of Management and Budget, and the Department of Health, Education and Welfare. He also has engaged in general law practice in Washington. Secretary Taft received his undergraduate degree from Yale College and his J.D. from the Harvard Law School, the latter in 1969.

KEYNOTE ADDRESS

*Significance of the
Joint Services Electronics Program
In the Overall U.S. Defense Effort*

The Honorable William H. Taft, IV
Deputy Secretary of Defense

It is a great privilege to join you at this symposium marking the fortieth anniversary of the Joint Services Electronics Program (JSEP). For four decades now, JSEP has been a key element in building and maintaining America's leadership in a broad range of electronics-based technologies. Together, you who represent university JSEP programs, the Defense Department, and industry--and your distinguished predecessors--have made a very real and significant contribution to this nation's defense.

The U.S. armed services created the forerunner of JSEP in 1946 to protect the unique national research resources that had been drawn together during World War II and had contributed so much to the allied victory. Farsighted leaders of that time recognized that science and technology had altered forever the character of international conflict, and the basis upon which world peace could be maintained. JSEP's birth coincided with the emergence of a national security strategy to counter our adversary's numerically superior military forces with superiority in science and technology. Since then, we have successfully achieved high confidence in our smaller deterrent forces by equipping them with vastly superior military systems. JSEP was part of the foundation of this strategy for using America's scientific and technological genius against an adversary's brute strength.

That the United States is still secure, that Americans are still free and our allies protected, is sufficient evidence of the wisdom of this decision and the enduring strength of our superior scientific and technical capabilities. Further, it is a testament to the success of JSEP,

to the creative expertise of the scientists and engineers who worked on JSEP programs, and to the effectiveness of the partnership between the Defense Department, our nation's major academic institutions, and the research facilities of America's defense industries.

This partnership was essential to our success in the second world war and continues to undergird security today. Certainly JSEP, which was the first peacetime attempt to use a DOD-academia-industry partnership to meet our nation's defense research needs, has been successful.

The program's achievements can be measured in many ways. It could be measured by academic prizes won. A quick look at JSEP history will reveal that sponsored researchers have been recognized with the most prestigious of prizes, including the Nobel Prize, and a variety of other national and international awards.

Or, we could measure JSEP by its growth. In this case we would see that the original four universities--Stanford, MIT, Harvard, and Columbia--have been joined by ten additional schools with active JSEP research programs underway. Further, we would find that JSEP has spawned additional opportunities for research and growth at several institutions, for example, at Columbia, where the astrophysics lab traces its history directly to JSEP research.

Or perhaps we could measure JSEP success by the results achieved in past programs. In this case we also would have a long list to draw upon. We could look at the evolution of radar from its rather crude beginnings as an

echo-sounding system to the sophisticated space-tracking systems of today; at Loran and other much more precise navigation systems; at the amazing accuracy of guidance systems for intercontinental and space flight; at the exponential development in computers. In each case, we would find that JSEP-sponsored research figured prominently in founding the technologies required for the sophisticated systems of today. Further, some of the most diverse developments in non-defense hardware that are today considered commonplace, from space systems to microwave ovens and home computers, were made possible by the early work of JSEP researchers.

We also could measure the success of JSEP pragmatically in terms of whether its sponsors--the Department of Defense and the military services--benefitted sufficiently from the program to continue supporting it. And you all know the answer to that.

In each of these cases, the bold experiment in partnership that we now call JSEP must be judged an unqualified success. But none of these accurately measures the magnitude of the JSEP contribution to the nation. The true measures of this program's success are 40 years without a major war between the world's most powerful nations, the preservation of America's freedoms, and the protection of our allies. JSEP's greatest achievement is found in our ability to deny adversaries any exploitable military advantage that could force concessions from the free world.

These are the real measures of JSEP's success. And these are the reasons that we in the Defense Department continue to be such strong supporters of our nation's

technology base. And today, the need for JSEP and other technology base efforts is more important than ever.

The threat has not diminished. In fact, the challenge to America's technology base has grown as the Kremlin benefits from its massive investment in military forces--including research and development. Not only have they continued to produce military hardware at a much faster pace than we and our allies, but also they have dramatically out-invested the United States in all research and development activities in the last two decades. In fact, a Commerce Department study published last year showed a vast gap between U.S. and Soviet support for R&D. Compared to when both nations made roughly equal investments in all R&D, the study found the Soviets increased the share of their GNP supporting R&D activities by some 80 percent through 1981, while the United States was reducing its support of R&D by about 5 percent. Just as important, the U.S. government was increasingly allocating its scarce research dollars to nondefense work. For example, in the early years of JSEP nondefense R&D was funded at one-third less than defense R&D; by 1980 defense and nondefense R&D were funded about equally.

Unfortunately, at the same time, the Soviets also were mounting a massive campaign to subsidize their own military R&D with illegally acquired Western technology. Their success in this effort yielded progress on a variety of new technologies--computers, heat-shielding materials, and circuit board production techniques. The result of these highly adverse trends was a threat to America's technical and scientific superiority, which underwrites national security.

The Soviet military machine was being equipped with sophisticated capabilities rivaling our own and, in some militarily useful technologies, they slipped ahead of us.

Even with the increased investment in R&D under President Reagan, we are still severely challenged to stay ahead of the Soviets. In fact, in our assessment of the 20 most important basic technology areas, there is relative equality between the United States and the Soviet Union in six vital areas--aerodynamics and fluid dynamics, conventional warheads, directed energy, nuclear warheads, optics, and mobile power sources and storage. In addition, while the United States is superior in propulsion systems, radar sensors, and submarine detection, we believe the Soviets are gaining quickly.

Fortunately, the United States maintains clear superiority in other areas. Particularly in computers and software, where many JSEP resources are concentrated, and which actually make possible so many important advances in a variety of other critical areas.

Still, the trend toward reduced technical superiority presents an important challenge for JSEP and all who work in the defense technology arena. Rectifying the disparity between U.S. and Soviet investment in useful research and technology, and rejuvenating the research and technology base that underwrites America's national defense, have been very high defense priorities since 1981. President Reagan and Secretary of Defense Weinberger have recognized the erosion of America's R&D leadership. They acted quickly to restore support for our future-oriented programs. Defense

research and development program support rose quickly from about \$14 billion in 1980 to over \$30 billion in 1986. As a result, the adverse trends have slowed or reversed and a number of important projects were accelerated--among them the research on strategic defense technologies and computer development. These and the many others have allowed us to begin recovering the leadership necessary to ensure superior defense capabilities 20 or more years away.

This is, of course, essential. For our responsibility in defense goes beyond tomorrow. If future generations of Americans are going to enjoy the security that we have--if they are going to have the tools needed to preserve peace in a dangerous world--then we must invest now. Research today will yield the technologies of tomorrow and make possible the superior deterrent systems of the future.

And that is what JSEP is all about. Through JSEP research programs, we are able to put the nation's brightest and most capable scientists to work on the most difficult defense problems.

Perhaps even more important than the quality of the people involved in the program is the way JSEP has become a model of cooperation and communication in research. It pioneered the team approach to research; engineers and scientists in our academic institutions joining policy makers in government and industry's technologists in seeking answers to critical problems. This cooperative effort is increasingly important because JSEP's focus on electronics has applications in almost all areas of scientific investigation and useful military technology. The interactive team

approach ensures maximum use of JSEP program results throughout our defense community.

The partnership of America's universities and our laboratories with DOD and industry has been a most successful one. In fact, we have used it as the model for our most recent effort, the University Research Initiatives. And we look to its expansion in the future. We in the Defense Department will continue to encourage all members of JSEP to work more closely together. Cooperative efforts have a synergistic effect and enhance our collective productivity, allowing us to make the most judicious use of JSEP resources.

President Reagan has offered us a great vision of a future free from the threat of nuclear missiles--and you have developed the basis upon which we can confidently look toward the day when that vision is fulfilled. But getting there, especially in this time of constrained funding, will require even greater effort from all involved in programs like JSEP. Efficiency and cost-saving improvements and industry investments will be even more essential as R&D resources become more scarce.

So, I ask you today to recall why you are here. Not just to celebrate, but to continue your efforts in the nation's defense. In the daily grind of research, classes, and teaching, I know that it is very easy to overlook the noble and important purpose to which your efforts contribute--world peace and the security of the free world.

As you review the many outstanding accomplishments of JSEP and share your plans for future research, I ask you to

bear in mind that you are shaping the security of future generations. To remember, as President Franklin Roosevelt so eloquently said, "We build and defend not for our generation alone. We defend the foundations laid by our fathers. We build a life for generations yet unborn."

History of the Joint Services Electronics Program

Arnold Shostak
ANSER
Former Chairman, Technical
Advisory Committee, JSEP

Your speaker is no Herodotus; but, armed with factual information providently supplied by university and government people (some living, or barely so; others departed from this mortal coil), I am in the privileged position of giving this distinguished audience an abbreviated history of the U.S. Armed Forces Joint Services Electronics Program (JSEP).

In that connection, we must not forget those individuals who have passed away, who were among those with the profound foresight and interest to launch and pursue this important program. Among those must be mentioned Zahl, Terman, Zacharias, Mason, Heffner, Pettit, and Silver. Of course, there are many others. Also, while we are in this vein, let no one at this symposium be offended by some inadvertent omission of his role or work in JSEP; since so many have been involved, it is impossible to give all due credit.

As you know, and as will be repeated on occasion at this affair, JSEP was conceived and initiated by forward-looking administrators in U.S. universities and the federal government, as an effective continuation of the World War II university-based research programs that had been established to advance our country's warfare capability in that period.

The effort was a joint one, as it was realized that fundamental investigations and advances in physical electronics, wave propagation, new devices, the mathematics of circuit and information theory, and advances in a broad field of applied physics, would most assuredly contribute

to all components of military, and, indeed, to civilian technology. This contemplation was surely prophetic and sound, as is manifest by the stream of research products, including personnel, which has resulted from the program.

The Electronics in the JSEP's name derives essentially from the fact that much of the early momentum in its establishment came from the U.S. Army Signal Corps. In fact, the program always has been multidisciplinary, as in applied mathematics, practical physics, and, in recent years, strong in information and coding theory and techniques, and in electronic devices and systems.

JSEP has many virtues, appealing to its Technical Coordinating Committee as well as to the university investigators themselves. Among these must be mentioned the long early funding--a feature that allows laboratory directors to plan ahead, and to allow some new direction of research.

In addition to support of students, JSEP also has served as a base for encouraging foreign faculty and graduates, who have trickled back to their native countries where they have assumed positions of leadership in research, engineering, and production of high technology, bolstering the economies and defensive strengths of the free world. Dr. Sanai Mito, a Harvard graduate, to whom the U.S. Navy granted a contract to pursue Barkhausen oscillator research at Osaka City University in Japan, is one example. Years later he popped up as Chief Engineer for Sharp Electronics of Tokyo.

Another case that might be cited is that of Dr. Hans

Schmitt, one of Prof. King's colleagues at Harvard, who became Director of the Phillip's Laboratory in Hamburg, Germany. (Incidentally, Schmitt, while with the King group, did a series of studies and experiments in the interaction of acoustic and radio frequency waves, following investigations of Peter Debye of Cornell and Karl Herzfeld at the Catholic University of America--thus advancing the area of surface acoustic wave technology, now so widely used in communications and electronic countermeasures systems.)

A clearly visible impact of JSEP activity at university centers has been the evolution of industrial activity, usually not far from those centers, entrepreneured, manned, and carried forward by students who have gone into those industries. Well-known examples are those centers on old Highway 128 in the Boston area, nurtured by MIT and Harvard and the one at Silicon Valley and elsewhere in the San Francisco bay area, activated by JSEP personnel from Stanford and the University of California, Berkeley. Similar nucleation and advance may be seen around the Georgia Institute of Technology, the University of Southern California, the University of Illinois, and Polytechnic University, just to mention a few. It must again be emphasized that JSEP must not be credited for all these inputs into the national economy, but certainly, the program has been instrumental in the phenomenon of this spectacular growth.

Ebb and flow have been distinguishing characteristics of JSEP. The program was originated by the Army and Navy, then joined by the Air Force when that Service was established. In the course of its existence, there have been times when government or university leaders have questioned

its soundness, and, indeed, there have been instances when some schools have withdrawn from the program. On occasion, there were pressures from one Service or another to curtail or terminate, partially or in toto, the joint venture with questions such as, "Why should the Navy give monies in support of Air Force research?" or vice versa. Through these vicissitudes, JSEP has survived and grown. It became clear to all involved that the basic research pursued under its sponsorship was paramount in the evolution of technology on which was based the advancing effective weapon and operational systems of all Services. Of paramount and unquestioned merit in the program is its "laissez-faire" approach, leaving to researchers how best to proceed in research. It must be admitted that there has been an entropic trend towards laying management layer on top of layer in all government (and presumably, industrial) laboratory activities. But, relatively, JSEP has maintained its idealism and kept micro-mixing-in to a minimum, considering the constraints imposed by your legislatures and the course of history.

I will not recount nor describe the numerous spectacular research products generated through the years by JSEP investigators, except where such reference will aid in viewing the historic flow. It should be pointed out that, generally speaking, it is not appropriate to claim single sponsorship for any individual contribution as that of JSEP's, since even shortly after its inception the tri-Service program was supported by associated correlative contractual sponsorship. Sometimes this was to accelerate advances in a particular field--as in the case of Zacharias' cesium resonator clock. Sometimes this was for

the purpose of being a recorded co-participant and to be kept informed on research advances. But mostly, this was for the purpose of driving through the basic investigation phase in seeking the attainment of some practical technique, device, or system.

As the years went by, JSEP came to be more and more used as a core program, about which would be wrapped extension activities, leading to some specific requirement of the Army, Navy, Air Force, or Marines. Ultimately, the peripheral segments dominated the overall program, with the result that latter-day JSEP levels are quite small compared with total ongoing university research efforts. Nonetheless, the prestige of JSEP sponsorship, with its attendant benefits of available wide-latitude funding for unique or novel investigation (in which some professor or graduate student could try something new) and with its ability to encourage and fund travel to scientific interchange meetings throughout the world, as well as with its remarkable interdisciplinary character that enabled detailed equilibrium interchange among scientists of various fields (and, I might add, among various JSEP university groups), has made the program immensely attractive as a powerful and effective nucleus about which broad programs of research in the electronic sciences and related fields could be pursued.

Now finally some history; first related to World War II activities. In the beginning, there was the National Defense Research Committee (NDRC), set up by Congress in 1940 to establish wartime-oriented research centers, mostly in universities, to aid the War and Navy Departments in the development of new equipment and/or military concepts. Dr.

Vannevar Bush was director of this organization. Principal electronics research centers were at MIT, Harvard, Columbia, and the Brooklyn Polytechnic Institute.

At the MIT Radiation Laboratory, Dr. Lee du Bridge and staff concentrated on microwave radar--some 69 different academic institutions participated in the work of this laboratory.

With reference to that laboratory, Prof. Julius Stratton has advised that magnetrons were provided to the du Bridge team by Sir Watson Watt's British group for the purpose of developing radar.

At the Harvard Radiation Laboratory, emphasis was directed to countermeasure methods and other aspects of electronic warfare. Fred Terman, who later was a leader in Stanford University's JSEP program, was director of the so-called Radiation Research Laboratory, accompanied by Joe Pettit, Bill Rambo, Mike Villard, and others, many of whom returned to Stanford with Terman.

The Columbia Radiation Laboratory was established in March 1942, under a contract with OSRD, operating under the direction of Division 14 of NDRC. The program was designed to meet the immediate objectives of military planning. The primary assignment was to develop microwave components at a frequency range far above that available in previously developed devices. In establishing the Columbia wartime laboratory, it was believed that some NDRC work should be carried on away from the immediate Service pressure on the MIT Radiation Laboratory. The laboratory was organized

under the directorship of Prof. I. I. Rabi, who was assisted by Associate Director Dr. J. M. B. Kellogg, during the war years, 1942-46.

The urgent need for improved radar performance during World War II also prompted the establishment of a Microwave Research Group (now the Microwave Research Institute) at the Brooklyn Polytechnic Institute (BPI) by OSRD. Dr. Ernst Weber was named Official Investigator of the BPI contract. In the framework of this contract, the institute interacted during the war with the MIT Radiation Laboratory. Principal contributions related to microwave measurement techniques and to the invention and development of basic components for microwave systems, such as attenuators, connectors, and power meters.

Now let us take a quick look at the birth and early thrusts of the JSEP laboratories themselves as they made the metamorphosis from wartime origins. The transition was not always smooth. To illustrate this I turn to the memoirs of our friend Harold Zahl as he referred to a problem that arose not only with JSEP contracts, but indeed with military contracts supporting research in universities. It came to pass, one time, that as part of a cyclic economy move within DOD (whose period is gradually decreasing), the Secretary of Defense ruled that no current fiscal year monies could be applied against time in the next fiscal year. Schools generally placed their staff contracts early in the calendar year, but under this new ruling, no school would know until the first of July whether or not their contract would be extended.

Acting for the Technical Advisory Committee (TAC), Zahl wrote to nine senior staff members of leading universities, asking for their reactions to such a ruling. They all replied, many at great length, stating that their schools could not operate under such a ruling--that is, to conduct research for the DOD. Dr. Zahl took those letters to the Assistant Secretary of the Army. He shook his head sadly and said the matter was out of his hand. Then it was when we played our "Ace" card. Dr. Rabi at Columbia, in his letter on the subject, had said, "If worst comes to worst, let me know. Maybe my 'Boss' can help."

Zahl called Dr. Rabi and asked him to try. Forty minutes later, Dr. Loughridge (Chief Scientist, Department of the Army) called and said that the Secretary of Defense (Charles Wilson) had recalled his ruling. What happened was that Rabi's "Boss" was Dwight D. Eisenhower, then President of Columbia. It took only a telephone call from him to the White House, and the problem was solved by a second call to DOD, that call from the White House from a gentleman named Harry Truman.

This quick look has been primarily concerned with those institutions that came into the JSEP fold early in the program. This is not meant to slight those who have come more recently--we seek only to show how the whole thing came about.

The following are brief histories of JSEP at each university. Much more comprehensive accounts can be found in the hardbound book, "40th Anniversary of the Joint Services Electronics Program," published for the symposium.

Massachusetts Institute of Technology

To effect the closing out of the radiation laboratories at war's end, a DOD Committee of three was appointed in March 1946. The committee was given very broad authority in the closing out process. It was made up of Lt Col Zahl (Army), CDR Piore (Navy), and Maj Marchetti (Air Force).

This committee took early action to divide millions of dollars worth of equipment based on the following priorities: first, to the Armed Forces Laboratories on a selected basis; second, to universities throughout the United States; and third, some was left at the Massachusetts Institute of Technology (MIT).

Then, to keep the MIT residue operational in military interests, on Army initiative a JSEP request was initiated at MIT to keep a relatively small cadre going in long-range research of military interest. Backed by the Navy and the Air Force, the Signal Corps contract was negotiated with MIT, Department of Defense supervision to be given by the Zahl-Piore-Marchetti Committee. Dr. Julius Stratton was named the Director of the new lab, called the Research Laboratory of Electronics (RLE). In that timeframe, it was not a question of "How much money?" rather the question was, "How much could be used effectively?" The initial contract was to cover a period of 2 years, all research to be unclassified. Prof. Julius Stratton was named Director of RLE, whose modus operandi can be inferred from the following extract, taken from the Final Report of that laboratory dated 30 June 1946.

"In the years 1943 and 1944, the Administration of the Massachusetts Institute of Technology set aside funds and laid plans to establish a Research Laboratory of Electronics as soon as the end of the War permitted it. This Laboratory was to act jointly with the Departments of Physics and Electrical Engineering to further the research work of the Institute in the broad field of electronics. As such the Laboratory may be viewed as a facility of the Institute where any staff member or student (primarily graduate) may carry on personal research which properly lies in the general electronics field. While the administration of the Laboratory is the responsibility of the Departments of Physics and Electrical Engineering it was early realized that other departments, especially those of Mathematics, Chemistry, and Biology, would be interested in the work of the Laboratory and the use of its facilities."

Shortly after V-J Day, Division 14 of NDRC, acting in accordance with the expressed wishes of President Truman, set aside funds to continue until 30 June 1946, basic research in the Radiation Laboratory at approximately a peacetime rate, with the hope that a permanent agency would take over support of this research after that date. The nature of the work of the Radiation Laboratory was either basic research per se, or the application of electronics to problems in science and engineering. Accordingly, the Director of the Radiation Laboratory established the Basic Research Division (of the Radiation Laboratory), and turned over its administration to the Research Laboratory of Electronics. Quite naturally the administration and most of the personnel of the Electronics Laboratory were members of the wartime Radiation Laboratory.

Shortly after the first of the year (1946) negotiations were begun among three Service organizations--the Navy Office of Research and Inventions, the Army Air Corps, and the Signal Corps--and MIT, to continue the work started under OSRD auspices. Accordingly, the mutual interests of these three Service organizations culminated in a contract between the Signal Corps and MIT for the continuance of RLE at approximately the same rate as the OSRD support. The participation of the three Services was equal, but for contractual convenience the legal arrangement was between MIT and the Signal Corps only. A technical advisory committee to the laboratory was set up consisting of the following Service representatives: Mr. John Keto, Army Air Corps; Dr. Harold A. Zahl, Signal Corps; CDR E. R. Piore, Office of Research and Invention (ORI). It was the purpose of this committee to meet regularly with and to advise the personnel and management of RLE on the technical administration of this contract. (The Synchrotron and Cyclotron Projects, at one time in the RLE envelope of research, were transferred to the MIT Nuclear Laboratory on 1 July 1946.)

Also at MIT, the Laboratory for Insulation Research (LIR) under Prof. Arthur Von Hippel, had been established as a separate entity in the Physics Department. This had been authorized by Prof. Compton as early as 1937. During World War II, LIR also was supported by NDRC with primary emphases in such areas as wave-guide design and the properties of solid materials. In the period after World War II and running to 1946, Von Hippel's laboratory was funded principally by the Physics Branch, Office of Naval Research. Prof. Von Hippel continued as director, assisted,

among others, by Dr. Richard Adler. Work at this laboratory was then continued under JSEP sponsorship, beginning in 1946 and phasing out in 1965, when the Center for Materials started at MIT.

Columbia University

I quote here from the memoirs of Dr. Harold Zahl (Army Signal Corps): "In late 1945, Lt Col James McRae and I met with Dr. Rabi of Columbia on the question of whether Rabi would be interested in converting the work of Columbia's wartime laboratory into long-range unclassified research of military interest. While very interested, he was very afraid that there might be too much military control. We offered him \$500,000 per year, and he shook his head, saying, 'Much too much.' We settled on half that amount with the stipulation that the Columbia Radiation Laboratory's (CRL's) activities would be self-developed but broadly in consonance with Army interests. The contract was originally exclusively Signal Corps, but was soon joined in support by the Navy, and a little later also the Air Force."

The laboratory was organized under the directorship of Prof. Rabi, and associate director Dr. J. M. B. Kellogg, during the war years, 1942-46. Prof. D. P. Mitchell assumed the directorship from 1946-50, and was succeeded by Prof. C. H. Townes, 1950-52, Prof. P. Kusch, 1952-60, and Prof. R. Novick in 1960. Others have followed.

Of significance in the history of CRL is the problem

that arose in the summer of 1944, when tests were made by CRL (still under NDRC auspices) indicating that the performance of experimental airborne K-band radar systems was not up to expectations. It was known that a water vapor absorption line was located in the general vicinity of the wavelength used (1.25 cm), and there was some experimental evidence tending to show that the water vapor content of the air was limiting the useful range of these systems. The laboratory was instructed to devise and perform an experiment that would locate and map out this water vapor absorption line. The ensuing investigation revealed that water vapor does indeed have an absorption line reaching its maximum at 1.3 cm. The experimental techniques developed were the beginning of important later work in microwave spectroscopy.

After the war, CRL turned its attention to a study of the fundamental physical properties of atomic and molecular systems. The microwave techniques that had been acquired were applied in experiments leading to a more complete understanding of hydrogen-like atoms. One of the first experiments was the measurement of the hyperfine structure of hydrogen, as well as the fine structure of this atom. Studies in the discrepancies between theory and experiment led to elucidation of the "Lamb" shift (by Willis Lamb of CRL).

A program was begun in 1951 to generate microwave oscillations by stimulated emission from excited molecules. The plan, which led to what was later named the maser, was based on sending a beam of ammonia molecules through a highly nonuniform electrostatic field.

Work on solid-state masers was begun in 1957, making use of electronic energy levels in various crystals cooled to the temperature of liquid helium.

The first masers that were used in radioastronomy observations were designed and constructed at CRL. They were mounted on the antenna of the Naval Research Laboratory and have yielded a significant body of data on planetary temperatures and on properties of radio sources.

Polytechnic University
(Formerly Brooklyn Polytechnic)

Just after the end of World War II, the Microwave Research Group, which had been working in adjunct to the efforts at MIT, Harvard, and Columbia, was formally named the Microwave Research Institute (MRI), a research department affiliated with the Electrical Engineering Department, with Dr. Ernst Weber serving as both Electrical Engineering Department Head and Director of MRI. Dr. N. Marcuvitz, who was at the MIT Radiation Laboratory during the war years, returned to Polytechnic in 1946. By the late 1940s and early 1950s, MRI had received considerable contractual support for its research from agencies such as ONR, the Bureau of Ships, Army Signal Corps, Rome Air Development Center, and the Air Force Cambridge Research Center. The program already was comprehensive in the electronic sciences, including many aspects of microwave research, and was expanding to encompass topics such as coding, information networks, electron tubes, and nonlinear magnetics, as well as electromagnetics.

The vision of Dr. Weber led to a novel symposium series on topics at the forefront of the electronic sciences, many of these sponsored annual meetings directed by JSEP. Proceedings of all 24 of them were published by Polytechnic Press, with Mr. J. Fox as editor. JSEP was established at Polytechnic University in 1956, under the broad theme of transmission systems research. This program extended the already ongoing research sponsored by individual components of the armed services.

Prof. Weber was the founding director of JSEP at Polytechnic University. In 1957, Dr. N. Marcuvitz took over the MRI directorship. The scope of the research of the institute was expanded during the period to include electronics-related contributions from other departments, a trend that was accelerated further after Dr. A. A. Oliner became the third director in 1967.

Electromagnetics and microwave techniques remained the principal strength in the program, and it is that area that has produced the greatest impact on the electronics field. Not only did MRI produce many important research results in electromagnetics and microwaves, but it also trained a whole generation of microwave engineers.

Consistent with the origins and the early history of MRI, many of the more significant results of JSEP-sponsored research were in the field of electromagnetics, but highly important contributions were also made in other areas, such as network theory, surface acoustic waves, x-ray diffraction, thin film and surface physics, control systems, image processing, and electromechanical power conversion. For

most of these accomplishments, recognition was achieved in the form of prizes, awards, or the publication of books.

Harvard University

Near the end of World War II after the Officers' Training Course at Harvard had ended, its director, Prof. E.L. Chaffee, enlisted members of the former teaching staff in a new organization that for a time had the name Central Communications Research. Scientists from the Radio Research Laboratory at Harvard and the Radiation Laboratory at MIT, which laboratories were being terminated, joined the new research staff. This was organized in three groups. The first, under Prof. Chaffee, pursued research on microwave and millimeter-wave generators; the second, under Prof. H.R. Mimno, worked on wave propagation in the ionosphere and radio aids to navigation; while the third, under Prof. R.W. King, investigated problems in electromagnetic radiation and antennas. Toward the end of 1945, Prof. Chaffee interested the Office of Research and Inventions (changed to the Office of Naval Research in 1946) in supporting and expanding the research effort. This was formalized early in 1946. Before the end of the year, the Army Signal Corps joined ONR in supporting the work. The Air Force completed the triad in 1948. Prof. Chaffee continued as director of the Harvard program until his retirement in 1953, when the administration was taken over by a committee chaired by the Dean of Applied Science, J.H. Van Vleck. Subsequently, Prof. Harvey Brooks, aided by F. Karl Willenbrook, lead the

group (this arrangement continued until Prof. Bloembergen became JSEP Director in 1966).

One of the notable members of the early staff was Prof. Leon Brillouin, who served on the Harvard faculty for several years. Also in the group was Dr. David Middleton, who became a pioneer in the development of statistical communication theory, continuing work started with Van Vleck during World War II. In 1949, a junior fellow, Nicholas Bloembergen, joined Chaffee's group to study the interaction of microwaves with magnetic materials.

Prof. Mimno's group included a number of former members of the wartime Radiation Laboratory, among them, J.K. Pierce. Pierce was a major contributor to the development of Loran (1941-46). During his long career at Harvard, with JSEP support he developed the Omega long-range navigation system. Prof. King led the JSEP electromagnetics group, which early on included D.D. King, who pioneered work on microwave measurements with JSEP support and C.H. Pappas. These men went on to distinguished careers as professors at the University of Michigan and California Institute of Technology, respectively. Prof. Erik Hallen (of the Royal Institute of Technology, Sweden), also an early member, was a pioneer in the integral-equation approach to antennas.

The program on electromagnetic radiation initiated by Prof. R.W.P. King has led to a large body of work on antenna configurations of all kinds. Both theoretical and experimental problems have been studied in depth. Researchers from the antenna group, for instance, S.R. Seshadri at the

University of Wisconsin, have proliferated all over the world as leaders in the electromagnetic sciences.

Dramatic evolution occurred at Harvard in the area of research in microwave electronics. In 1946 Prof. Chaffee already was aware that much progress could be expected from study of the interaction of microwaves with atoms, molecules, and condensed matter. When Dr. Bloembergen wanted to study basic ferromagnetic resonance phenomena at microwave frequencies in 1949, Chaffee gladly encouraged him. (Bloembergen had been a graduate student with E.M. Purcell and had worked on fundamental studies of nuclear magnetic resonance.) The importance of magnetic resonance phenomena for electronic devices soon became clear. Prof. C.L. Hogan joined JSEP and started a research project on ferromagnetic and ferroelectric materials, later taken over and expanded by Prof. R.V. Jones. This program contributed a great deal to microwave devices, such as isolators and circulators, now widely used in radar and other microwave installations. At the same time, Bloembergen's group demonstrated the existence of different spin temperatures in a magnetic system, the principle on which he developed the three-level solid-state maser. This invention made possible extremely sensitive microwave receivers, which were used in the early DEW line radar defense as well as in large radio telescopes. Without making any formal proposal for a change because of the freedom provided by JSEP support, Bloembergen then pursued the same type of physics into the optical region, where he opened up the field of nonlinear optics, for which he received the Nobel Prize in 1981.

In parallel with these developments, the effort in solid-state electronics was broadened in 1953 to include work in the burgeoning field of semiconductor physics by W. Paul and in solid-state theory by Harvey Brooks. Considerably later, in the early 1970s, a new activity in superconducting electronics was added, led by M. Tinkham.

As mentioned earlier, larger numbers of research personnel have come from the JSEP-supported research at Harvard, to participate in laboratories, industry, and government throughout the free world. The program at Harvard is now under the direction of Professors Bloembergen and Tinkham.

Stanford University

The JSEP program of the Stanford Electronics Laboratory originated in work previously done under four separate ONR contracts. In 1947, these contracts were consolidated into a single contract with added support by the Army and Air Force to establish JSEP at Stanford.

The ONR program was in the area of radio propagation, with work on meteor reflections, low-frequency propagation, ionospheric sounding, microwave electron devices, and wide-band network studies, the last under the direction of J.M. Pettit. This research was continued under JSEP sponsorship. Many of the faculty and students in this early period had come to Stanford as a result of their work in radar and countermeasures systems with Terman at the Harvard Radiation Research Laboratory.

The broad objective of JSEP at Stanford was to support and encourage basic research in electronics (of a character suitable to a university) that would provide new ideas and new data on electron devices, electronic systems, and electronic phenomena of possible interest and importance to government and industry and that would simultaneously train a new generation of electronic research scientists to fill the obviously great needs of government, industrial, and university laboratories.

Microwave/Ginzton Laboratory

The JSEP program in the Microwave/Ginzton Laboratory was started in 1950. Its planned objectives were the logical continuation and extension of an ONR program on high-powered, microwave amplifiers that was already underway in that laboratory. The activities of the laboratory were originated in 1945 by Prof. William I. Hansen, who had returned to Stanford from wartime leave at Sperry Gyroscope Company. Prof. Hansen was the inventor of the cavity resonator. He was also one of the co-inventors (in 1937, with the Varian brothers) of the klystron. Hansen had worked at Sperry during the war largely on microwave problems, radar, and active and passive devices, and was one of the world's greatest experts on all aspects of microwaves.

Prof. Hansen was joined in his efforts by two former colleagues from Sperry--E.L. Ginzton late in 1946 and M. Chodorow in early 1947. Ginzton had worked with the Varian brothers and Hansen on the development of the klystron at

Stanford from 1940-41. From both the Stanford Electronics Laboratory and the Ginzton Laboratory have flowed a torrent of people, ideas, and devices, all of which have had beneficial impacts in the world of science, industry, and government.

University of Illinois

The Control Systems Laboratory (CSL) at the University of Illinois was organized early in 1951 under the impetus of urgent military needs brought about by the national involvement in the Korean War. In its early phases, the Illinois research was funded by the U.S. Army Ordnance group in Detroit, as well as by ONR. In 1956, Professors F.W. Loomis, Fred Seitz, Andy Longacre, Nelson Wax, Charles Sherwin, and others, urged formation of a midwestern laboratory that would do for naval task forces what the Lincoln Air Defense Laboratory was doing for the Air Force and Army. This thrust was envisioned by Prof. Loomis, who had been part of the group that organized the Lincoln Laboratory. Loomis was named Director of CSL, with Fred Seitz as Technical Director.

I can recall going down to the Marine Corps Equipment Board with Fred Seitz. There he learned of the importance to the Marines of applied research in the application of incoherent pulse Doppler radar to Butterfly and Truckfly battlefield surveillance systems. These efforts were in addition to the main thrust at CSL, namely marrying the electronic digital computer to the height-finding radar,

with the early evolution of the Naval Tactical Display System (NTDS), a reconnaissance and air-traffic control system now widely used by our armed forces.

In consultation with the Joint Services Committee, the university administration in 1959 approved a recommendation of the laboratory staff to formally reorganize the laboratory into an interdisciplinary and interdepartmental graduate research center in the College of Engineering, and renamed it the Coordinated Science Laboratory, with Prof. D. Alpert as Director.

Many contributions have been made by CSL, then and thereafter. They include the electrostatically-supported gyroscope, interactive Plato computer learning methods, and an abundance of work in electronic components and systems, control theory, and in quantum electronics.

University of California, Berkeley

The Joint Services Electronics Program at the University of California, Berkeley began in 1961 by combining a number of existing grants and contracts from the Air Force, Army, and ONR. Professors John Whinnery and Samuel Silver were the leaders in forming the JSEP structure. The original programs receiving support were bioelectronics, integrated circuits, microwave antennas and radiation, microwave electronics, solid-state electronics and systems, and energy conversion and control. The first program director was Donald O. Pederson. In the formative years of

JSEP at Berkeley, the leaders included Professors P. Morton, E. Kuh, D. Angelakos (later director of the laboratory), J. Whinnery, T. Everhart, L. Zadeh, C. Desoer, and E. Jury.

The objectives of the first JSEP contract were to provide general support for a broad spectrum of basic research activity and to ensure that every qualified graduate student in electronics would be able to receive financial support. The program came at a critical period of development of electrical engineering at Berkeley and provided a needed financial base for the overall research program. Measurable output, such as journal articles, technical talks, and nucleation of major research activities, increased dramatically in the years immediately following the start of the JSEP program.

Over time, JSEP at the University of California, Berkeley, evolved to focus more sharply on those areas of direct interest to DOD. The original emphasis on microwave electronics, for example, has evolved to include research in integrated circuits. Component research has evolved into an activity in computer aided design (CAD) of integrated circuitry, an area that continues to grow in importance. The microwave and antenna segment continues to be a strong activity. JSEP also was instrumental in establishing a viable bioelectronics program.

Throughout its history, JSEP at the University of California has involved a stream of new, young investigators. This has kept the program extremely dynamic in research of the electronic sciences.

University of Southern California

The fast pace of growth in aerospace, avionics, and governmental activities in Southern California made manifest the need for augmented university-based research, especially in the electronic sciences and related fields. Hughes, Northrop, Rockwell, the Naval Electronics Laboratory, (Point Loma), and the Naval Weapons Center (China Lake), are among regional activities that have profited from studies and available personnel from the University of Southern California (USC) JSEP activities. This program was initiated by Prof. Zohrab Kaprelian, aided by Dr. Jack Munushian, in 1963.

JSEP at USC was an outgrowth of the Air Force Office of Scientific Research (AFOSR) supported research in artificial dielectrics, soon to be followed by a wide range of investigations in electronic materials, solid-state research (including work in semiconductors, magnetism, crystal imperfections, and superconductivity), information systems (including control systems, bioelectronics, and coding theory for communication systems), and electromagnetic research (including studies of the properties of plasmas, wave propagation through various media including plasmas, and electromagnetic waves in stratified media).

JSEP at USC has served as a nucleus about which much defense and civilian economy-oriented research has been pursued, to the advantage of not only Southern California, but to the entire country's scientific community.

University of Texas

JSEP was established at the University of Texas at Austin in 1964. It was administered by the Laboratories for Electronics and Related Science Research (later renamed the Electronics Research Center), with Prof. Arwin A. Dougal and Prof. A.H. LaGrone serving as Director and Associate Director, respectively. In one sense the program grew out of AFOSR and ONR contracts in quantum electronics (under Prof. Dougal) in the earth radio sciences (under Dr. Harold Smith), and in radio wave propagation studies (under Prof. Archie Straiton).

JSEP at the University of Texas has been dynamic, in that both the faculty involved and the focus of the research have continually evolved over the years. Dr. Arwin Dougal served as Director from 1964 to 1967, whereupon he accepted an assignment at the Pentagon as Assistant Director of Defense Research and Engineering (Research). Dr. C.L. Coates, former Chairman of the Department of Electrical Engineering, served as Director from 1961 through 1971. Dr. Dougal again took over the directorship in 1971. In 1977, Dr. Edward J. Powers was appointed Director. Approximately 60 faculty members have participated in JSEP, the majority drawn from the Departments of Electrical Engineering and Physics. For many of the faculty, JSEP has provided seminal support for programs that were ultimately spun off and supported by other DOD and/or federal agencies. It always has been the policy of JSEP at Texas to have a mix of faculty participants, i.e. both multidisciplinary and a span of experience ranging from new assistant professors to highly productive senior professors. This

mix has contributed greatly to the vitality of the Texas program.

In recognition of numerous accomplishments in research many of the JSEP faculty have been elected Fellows of the Institute of Electrical and Electronics Engineers or Fellows of the American Physical Society. In addition, their technical expertise and leadership have been recognized in other ways; many have served as editors and on the editorial boards of many of the top U.S. scientific and technical journals. Other recognition came in the form of awards, such as the Pattern Recognition Society's 1975 best paper award, presented to Drs. J.K. Aggarwal and J.K. McKee for their work entitled "Finding the Edges of the Surfaces of Three Dimensional Curved Objects by Computer."

From the JSEP laboratory at Texas has flowed a stream of research accomplishments in pattern recognition, biomedical engineering, solid-state and thin-film electronic research, electromagnetics, and quantum electronics. Many graduate students who received JSEP support have become associated with industry, universities, and government.

Ohio State University

Ohio State University (OSU), a leader in electronics research, aware of the value and importance of JSEP sponsorship of their work, had sought association with JSEP as early as 1960. At that time, Congressional interests in university-funded research had developed in the direction

of project Themis, a well-intentioned scheme to distribute funds for basic and applied research throughout the land, at large universities or at rurally located institutions. The rationale was that homogeneous broadening of the nation's effort would be advantageous. By some measure, this plan did not work out, primarily because the principal centers of intensified multidisciplined investigations already had attracted most of the good people, and, in addition, those institutions were up-to-date in the frontiers of research--knowledgeable in what had to be done next to advance some field. Be that as it may, Themis squeezed out Ohio State's opportunity to join JSEP at that time. (That is not to say that Ohio State was one of the rural group--not at all--its reputation as a center for advanced studies in basic and applied electromagnetics was already known throughout the world.)

When Technical Coordinating Committee funding enabled consideration and subsequent incorporation of OSU into the SEP fold, it became a member of the group, in 1977, with an excellent record of accomplishments and contributions.

Cornell University

The JSEP program at Cornell was established in 1977. The original investigators were all members of the electrical engineering faculty: Eastman (Director), Lee, Ku, Frey, McIsaac, Dalman, and Carlin. In the first years, the program was aimed at several aspects of microwave research--semiconductor materials, devices, and circuits.

The device work consisted of research FETs and variations for microwave amplification and generation.

In JSEP's 9-year existence at Cornell, the original research on semiconductors for microwave applications has progressed and other related areas of research have been added. Eastman remains as director with Wicks, Woodard, Shealy, Ballantyne, Tang, Ju, Carlin, and Krusius as contributors. The semi-conductor materials work is now concerned with more versatile and precise growth techniques. Transistors under study are more complex and capable of higher frequency operations. Newer related areas of research in JSEP at Cornell include high-speed semiconductor lasers and optical detectors, and studies in GaAs similar materials.

Cornell JSEP research products and personnel have made their mark in aiding the country's posture in advanced defense systems, as in phased-array radars, as well as in industrial and consumer products such as data systems used in telephony.

Georgia Institute of Technology

The JSEP program at Georgia Tech may be said to be a derivative of the work at that institution originally supported by the Army Research Office. This work was in signal processing, systems, and information theory. In 1980 when funds became available, the Georgia Tech program, which had been tentatively arranged as an associate group

to JSEP, became a fully funded establishment in 1980. Professors D.T. Paris and R.W. Schafer were co-directors in that activity.

Much important research has been conducted, as in improved diffraction gratings, significant in holography and acoustic radio-frequency theory and devices. Application of various techniques, coming from the general area of signal processing involving electro-optical phenomena, has been put to use in advanced computation devices.

The theoretical concepts of cyclo-static parallel processing implementations are now being realized in hardware as a "DSP supercomputer" with support from DARPA. JSEP support of research at Georgia Tech has had a dramatic and measurable impact. Over half of the principal investigators have become Fellows of their professional societies on the strength of their JSEP-supported research. The existence of strong research programs in turn has made it possible to attract outstanding young people to the faculty of the school. JSEP support also has been a major factor in the dramatic increase in Ph.D. degrees granted by the School of Electrical Engineering in recent years.

Conclusion

You all know the special quality of military-sponsored research. It is certain that the U.S. military must maintain continuing contact with university scientific research programs so that any new or improved technique or device,

including the unexpected, may be quickly evaluated and possibly incorporated in the evolution of our arsenal of defensive weaponry. Leaving the task of transferring the products of basic research into usage by our armed services to scientific liaison personnel of civilian agencies is not enough; the transfer efficiency may be low. Accordingly, the role of organizations such as JSEP in effecting assured early awareness of the significance and utility of scientific advances, as reflected in the time-varying programs of our best university research in the electronic sciences, is clear and will be maintained. With support from all executive and legislative levels, the program will be continued and expanded where possible, in our nation's best interest.

EARLY JOINT SERVICES ELECTRONICS PROGRAM ACTIVITIES

Chaired by
Dr. Jimmie Suttle
Army Research Office
Technical Coordinating Committee, JSEP

*The Research Laboratory of Electronics at the
Massachusetts Institute of Technology*

Dr. Henry Zimmerman
Professor of Electrical Engineering
Massachusetts Institute of Technology

When I arrived at the symposium yesterday, the first thing that happened was that Arnold Shostak tapped me on the shoulder and said, "Jerry Wiesner, one of the speakers, is ill--you are giving a speech tomorrow morning." So, I put together some notes. I'm not sure it is a speech, but I'll try to make it as cohesive as I can.

You have all heard about the transition of the MIT Radiation Laboratory to a basic research group sponsored by NDRC, which subsequently became the Research Laboratory of Electronics (RLE). RLE started initially as an interdisciplinary laboratory jointly sponsored by the MIT Electrical Engineering and Physics Departments and supported by JSEP. The important thing to remember is that the MIT Radiation Laboratory provided us with an heritage of ideas that had accumulated during the war, when the emphasis was on producing radars and getting them out to the field. Many basic ideas that the physicists and engineers had developed were put on the shelf at that time for future reference. Those ideas gave us so many things to work with that it was not a question of how will we fill the time? but rather, what shall we do first? That was important. The other thing that was important was the heritage of equipment that we acquired.

Julius Stratton and Albert Hill started RLE, joined shortly afterward by Jerry Wiesner when he came back from Los Alamos. And there was one other person, a physics professor named George Harvey, who was part of that basic research group. His task was to put tags on the equipment that we wanted to keep versus that which was going to be shipped back at the end of the war. It turned out that

there were more tags than non-tags. We used to wonder what we would do with all of this equipment, but as time went on and the laboratory grew, it was all put to good use. But those two things, the ideas and the equipment, gave us a nucleus from which to start.

Now, what were the ideas? Microwave technology, which had developed during World War II, immediately gave the engineers a chance to do some things with antennas and wave guides that they had never done before because now they had a lot of components to work with. They had magnetrons, klystrons, and all kinds of wave-guide components and test equipment with which to work. Thus, they began to think about such things as frequency-stable systems. But this activity was really trivial compared to what the available microwave technology did for the physicists. The microwave instrumentation that was available gave physicists a chance to tweak atoms and molecules with low-energy quanta and study things that they had never been able to look at before.

Such a raft of things evolved, that it is hard to delineate them all. There was microwave spectroscopy, which eventually lead to some maser and laser development. There were some atomic resonance phenomena and molecular beam studies, which Dr. Zacharias started and which eventually led to the atomic clock. There were microwave gas discharge studies that started initially with investigations in very-low-pressure gases. This was pursued because the engineers were looking at communication problems. Drs. Sanborn C. Brown and William Allis did some microwave discharge studies using pressures that were similar to those

in the ionosphere so that one could get a feel in the laboratory for some of those communication problems.

These studies rapidly escalated into higher and higher pressure studies, eventually leading to the tremendous amount of work that was supported by AEC, and more recently, other agencies, on high-density plasmas. The various and sundry devices and techniques studied are too numerous to mention completely. Francis Bitter was doing nuclear magnetic resonance studies. Many of his ideas eventually led him to develop high-flux magnets. Thus, one of the spin-offs that we can claim from RLE is the National Magnet Laboratory, founded by Bitter. These were things that were going on in the physics side of the house.

On the other hand, the electrical engineers in the laboratory were interested primarily in communications and electronics. You have to recognize the fact that before World War II those fields consisted of telegraphy, radio, and a little bit of audio amplifiers (stereo had not been invented yet). There was no such thing as high fidelity--you tried to make an amplifier that would almost reproduce what you wanted to reproduce--but it was not easy. We were trying to make nonlinear systems as linear as possible.

Television was still around the corner. Mr. Hollis Baird, a local Boston man, had an experimental television system running. Some of our graduate students built receivers, which they kept in their dormitory rooms, and once a week for an hour in the evening, he would put on transmissions that they would try to receive. But all of the pulse technology that came out of radar during World War II was

a mystery to most of the electrical engineers. It had been classified, so if they had not been involved in radar, they did not know about it. Therefore, we began to teach pulse techniques and RLE began to do research along those lines.

One of the effects of that input from World War II was that Professors Guilleman and Cerrilo became interested in, and some of the graduate students shifted their interests from, the frequency domain to the time domain. The group began to do time-domain network synthesis. From these ideas eventually evolved much of the work on discrete systems, which in turn, led to pulse techniques in computers (of course, some of the early computers were not quite what we would recognize today). The engineers had equipment that they had never seen before. The Synchroscope, developed at the Radiation Laboratory for timing high-speed wave forms and viewing brief pulses, was a relatively new thing. Mechanical oscilloscopes, with which you could study power circuits, had been in use, but they had responses only up to 30 hertz. But the Synchroscope, which could operate up to the megahertz region, was completely new. This opened up a whole new field of research in communications. There had been some work done during World War II on pulse communication systems, pulse width modularization, pulse frequency modulation, and more importantly, pulse code modulation. The whole area of communication technology was rapidly developing at RLE.

Added to the idea of using pulses to convey messages were the very important notions of noise, and the statistical approach to communication theory. During the latter part of World War II, Norbert Wiener wrote a timely

treatise on time series. To most electrical engineers, it was completely Greek, until Professor Y. W. Lee developed a graduate course in which he interpreted Wiener's mathematics at a level that engineers could hope to comprehend and cope with. This led to development and use of statistical communication theory. The RLE engineers were learning terms such as auto-correlation, cross-correlation, and least-square-error criteria. This knowledge, in turn, led to the capability of designing systems in an optimal manner. (The least-square-error criterion is not always the right one to use, but it was the only one that was analytically tractable.) The pressure to be able to do this with other criteria and with pseudo-random signals, rather than completely random signals, led to a lot of work on specialized computer equipment to calculate some of the functions that were needed. Things evolved in a rather natural fashion, but rapidly.

Looking back now, the momentum that developed during the late 1940s and early 1950s is almost unbelievable. The communication theory group held a weekly seminar that would have 30, 40, or 50 people attending. They took turns talking about what each one was doing, and very rapidly the idea of extracting signals from the noise began to emerge. One of the first things they did was to examine a sonar record and pull out some signals that were completely unintelligible otherwise. The first thing that happened after that was the Defense Department wanted to classify the work. The argument against classifying this work was that the mathematics was public knowledge and anybody with a pygmy brain could deduce its application. Therefore, there would be no point in classifying the activity. Fortunately

it was eventually left unclassified--a decision that was in the best interests of the development of that field.

As Arnold Shostak pointed out, one of the things we have to be careful of is not to give the impression that everything was done in JSEP laboratories. There were many other places doing good work in the field and, of course, the Bell Telephone Laboratories was one of those. We cannot pass this occasion without mentioning Claude Shannon's work on information theory, which was done at those laboratories. Somebody (I believe it was Jerry Wiesner) talked Shannon into coming to MIT for a semester to teach a course and join the research group. Fortunately he decided to stay and work on information theory, which after his arrival blossomed very rapidly. Many of our best graduate students wanted to work in that field. Shannon was joined by Fano, Davenport, Elias, Gallagher, and others, all important names in the field.

Information theory, having established the maximum capacity of communication in a channel, led to coding theory, which offered a way to extend the upper limit. The way to beat the upper limit was to code the signal before it was put through the channel. That led to the whole field of coding theory. That technique is now absolutely indispensable to the transmission of digital data. Reliable digital data transmission evolved from work that was going on in the 1950s. And as I said, many other laboratories were working in the field and contributed to its growth.

The next thing I recall happening in the digital field was that a group came to MIT from the Bell Telephone

Laboratories to teach a course in combinational switching theory. A good many students attended and a lot of faculty sat in as well. One of the most interested was Professor Sam Caldwell, who took the material, organized it a bit, and started a course in that field. One of his graduate students, David Huffman, did a doctoral thesis on sequential switching theory, which formed the basis for much of the later computer design work. In that connection RLE had done work in analog computer applications. One such computer was an analog correlator for calculating auto- and cross-correlation functions very quickly (this was done by Wiesner, Lee, and Cheatham). As digital technology evolved that device was supplanted by a digital correlator developed by Henry Singleton, then a graduate student and later the founder of Teledyne. If you look to see where JSEP has led sponsored programs, you must, as well, recognize the accomplishments of those people who have participated in the program.

I probably have spent more time on the engineering side of the subject and less than I should on the physics. I do not mean to slight the physics side, but I thought probably Townes and Bloembergen would cover more of that field. In the time available it was not possible to cover everything that was going on, so let me just indicate a few of the numerical facts.

In 1946, when RLE started, there were five research groups: three of those were physics groups and two were engineering groups. There was a lot of interaction among them. One of the nice things we had was a little conference room with a big long table that was left over by the

Radiation Laboratory. This table had a fine oil cloth on it. We also had a little lunch counter where you could get sandwiches and everybody bought lunch. We sat around the table and talked shop during the whole lunch hour. A great deal of interchange of ideas took place in that most informal and casual way. We also were located in the old Radiation Laboratory buildings, which had two stories--a basement and two floors. These were horizontal buildings, where you would pass people in the corridor. Such buildings are more conducive to intercommunication in an interdisciplinary laboratory than one would find in vertical buildings. Fortunately there is still a lot of activity in the old Radiation Laboratory buildings. They have been renovated and are full.

To go on with the statistics, in 1946, those five research groups included about twenty faculty members. The total body count in the laboratory was 100 or less. By 1941, there were 10 research groups, 40 faculty members, and a total of 300 people. So the research groups and faculty members doubled and the total body count went up by more than double. By 1956, after 10 years, we had 22 research groups again and 60 faculty members. At that phase we had a saturation problem in the Electrical Engineering Department; the faculty could not handle anymore graduate students, so the rest of the body count was about the same. Then, by 1961, which was 15 years into the program, we had 30 research groups, 120 faculty members, and a total body count of 700.

What caused that increase? The initial laboratory was mainly comprised of personnel from the physics and

electrical engineering departments. But by 1961, we had people from 10 academic departments within MIT participating in the research at RLE. There were chemists, mechanical engineers, aeronautical engineers, nuclear engineers, biologists, linguists, and psychologists. We were studying all aspects of the communication problem, from the conception of an idea in one brain to its interception in another brain. We had a Center for Communication Sciences, which flourished and led to many spin-offs like Project MAC, the Artificial Intelligence Laboratory. RLE provided central facilities: machine shops, drafting, and glass shops, whatever was necessary for research could be found at RLE. Faculty members who had grants that would support two graduate students and half a technician saw that they could get everything they needed at RLE. They were pounding on the door for admission.

That situation prompted Gordon Brown, then Head of the Electrical Engineering Department, to say, "If this continues, pretty soon all the research at MIT is going to be done at RLE. We have to recognize that this is a great idea and try to replicate it." He developed a concept of what he called a university polarized around science. Recognizing the importance to teaching of the interdisciplinary laboratory, he thought MIT should have many such interdisciplinary laboratories. His dream did come to pass because we now have a material science laboratory, thanks to ARPA, a space science laboratory, thanks to NASA, and many smaller interdisciplinary groups organized within academic departments. Thus, the idea that was started in RLE with JSEP support has proliferated.

I would like to point out how important the seeding is in the process. We all tend to put the emphasis on research, because that is what we were hired to do by the sponsors, but I think that we must recognize an equally important by-product, namely the impact of that research on faculty members and graduate students in the way of career development. There were things that could be done that would have been impossible without this kind of support. Research results fed back to the academic program, which led to new courses being introduced in the physics, electrical engineering, and various other departments. Many of these courses started as graduate seminars while the research was still in progress. There is no way you can stimulate a student better than with new ideas that nobody else has worked on. He wants to participate! So, one cannot underestimate the importance of the impact of these programs on the academic side of the operation.

And of course, if we really want to measure the value of the Joint Services Electronics Program, we have to trace the accomplishments of the alumni, both faculty and graduate students, who came from the laboratories in which JSEP provided core support. I have to mention the importance of core support. That gave us the freedom to start new things and gave us continuity as other grants came and went, thus literally leading to the survival of those laboratories. In an academic environment, there would be no way we could have done it without that factor.

At the end of World War II there was a need to continue and expand basic research, there were funds to provide the opportunity, and there were men who had the vision to act. There were undoubtedly many others involved but we are particularly indebted to John Keto, John Marchetti, Emmanuel Piore, and Harold Zahl, for their contributions to the initiation of the Joint Services Electronics Program.

Forty Years of JSEP at Harvard University

Dr. Nicholas Bloembergen
Gerhard Dade University Professor
Harvard University

Age is a relative concept and it is legitimate to ask why we celebrate any 40th anniversary, and in particular, why do we celebrate the 40th anniversary of the Joint Services Electronics Program? One reason it is legitimate to do so is because JSEP is indeed 40 years young and because many people active during the infancy of JSEP are still alive and are present here today. But we are not gathered here to compliment ourselves in mutual admiration for a job well done, however pleasant and justifiable that may be. The purpose of this meeting is, by reflecting on history, to reaffirm the identity and purpose of a viable program and to prepare JSEP for even greater challenges that lie ahead.

As I said at the beginning, it is good to keep the perspective that age is a relative concept. While JSEP is 40 years old, the Department of Defense serves a nation that is 210 years old and this year celebrated the 200th anniversary of its constitution. I represent a university which celebrated its 350th anniversary earlier this month. During these celebrations, it was emphasized that Harvard also prepares itself continually for future needs and challenges. I learned that the signers of the Declaration of Independence included eight Harvard graduates. Skipping two centuries, several current cabinet members, including the Secretary of Defense, hold Harvard degrees. Harvard is determined to do equally well or better in the future.

Harvard assisted the U.S. Navy in World War I by operating a radio school. Names such as G.W. Pierce and E.L. Chaffee were associated with that early collaboration. During World War II Harvard operated an Officer's Training

Course under the direction of Leon Chaffee. Faculty included R.W.P. King, D.D. King, H.R. Mimno, and L. Brillouin. These people, together with J.H. Van Vleck and others, also carried out work on radar countermeasures and noise at Harvard's Radio Research Laboratory, which complemented the large effort at the MIT Radiation Laboratory. As at MIT, the activity was housed in a wooden building.

Professor Chaffee, in late 1945, interested Admiral Furer, then Head of the Office of Research and Inventions of the U.S. Navy, to continue and expand support of the war-time research effort. This was formalized in early 1946 with Contract N5-ORI Task Order 1. Before the end of 1946 the Signal Corps of the U.S. Army joined the Office of Naval Research (ONR) in supporting the work. The Air Force completed the triad in 1948. Professor Chaffee directed JSEP at Harvard until his retirement in 1953. He achieved fame for his work on high-power vacuum tubes in the period between the two world wars and received the Medal of Honor from the Institute of Radio Engineers in 1959.

Initially, the JSEP effort at Harvard was divided into three groups. The first, under Chaffee, pursued research in microwave and millimeter wave generators. It evolved into other activities, about which more will be said later. A second group, under R.W.P. King, was, and still is to this date, concerned with electromagnetic radiation patterns and antennas. Professor King, although officially long retired, is still active and comes in regularly. This group is now directed by Professor T.T. Wu, a former student of Ronald King. He is one of the foremost theoreticians in radiative theories. One of his current interests

is in extra low frequency (ELF) communications and communications with submarines under water and ice.

The third group, under Professor Harry R. Mimno and Dr. Jack A. Pierce, worked on wave propagation in the ionosphere and radio aids to navigation. Starting from Loran work during World War II, they developed first a low-frequency system, called Radux, which was implemented on an experimental basis but never came operational. A second system, Omega, improved ways to reduce and eliminate transmission anomalies in the ionosphere as a result of solar activity. This low-frequency system used antennas transmitting from Hawaii and Wales. In 1973, Jack Pierce could say, "The Omega system combines the primitive virtues of worldwide and continuous coverage with excellent reliability...errors of not more than half a mile, and economy of operation remain unmatched in the history of modern navigational techniques." He received the Conrad award from the U.S. Navy in 1975. Of course, times have changed. Precision of half a mile is not good enough; the big antennas of Harvard's Cruft Laboratory have come down. Modern navigational techniques rely on atomic clocks, which beam signals from satellites in known orbits.

In 1949, I joined Professor Chaffee's group, which was still housed in the wooden Vansberg building. I wanted to get experience in microwaves, having worked previously with radiowaves and lumped LC resonant circuits in nuclear magnetic resonance. The handwriting was already on the wall that the focus of attention was switching from vacuum tubes to solid-state devices. I studied ferromagnetic resonance phenomena at high temperatures, around the Curie point, and

at high microwave powers. In 1951 we moved from the Vansberg building to the Gordon McKay Laboratory, then three stories high. Two stories were added in the early 1960s. It still houses most of the experimental part of Harvard's JSEP. C.L. Hogan, the inventor of the microwave Faraday isolator, joined JSEP in 1952. He was assisted by Prof. R. Victor Jones, who later took over the extensive work on ferromagnetic and ferroelectric materials.

A group effort on noise and stochastic problems, started by David Middleton, developed into the area of electronic decision and control. Arthur Bryson optimized aircraft trajectories for minimum time to climb. Currently, Professors Ho and Brockett are working on nonlinear problems in controls and estimation. The experimental solid-state electronics program branched out into amorphous semiconductors (Bill Paul), superconductors (Mike Tinkham), and liquid crystals and surface structure (Peter Pershan). In the program at Harvard there has always been a very close association between theory and experiment. Theoretical solid-state investigations were conducted in the 1950s by John H. Van Vleck and Harvey Brooks, and since the early 1960s by Henry Ehrenreich. Van Vleck and Brooks were deans of the Division of Applied Sciences at Harvard. They oversaw JSEP at Harvard from 1953 until 1966 with the aid of a steering committee, which included Dr. F. Karl Willenbrock. I served as director for the Harvard JSEP program from 1966 until 1983, when Michael Tinkham took over.

My own efforts in the 1950s were focused on magnetic resonance techniques, especially as applied to the materials and electronic structure of condensed matter. My group

did a lot of work on nuclear and electronic paramagnetic resonance. Two early collaborators were George Benedek, now a professor at MIT, and Dr. T. Kushida, now at Ford Motor Company.

This work culminated in 1956 in the proposal for a pumping scheme to produce a continuous-wave low-noise microwave maser. Arnold Shostak insisted that I make a trip to Washington to brief the military brass about it. Since I was still a Dutch citizen at that time and a novice at such tasks, I am afraid that this presentation was one of my less effective contributions to JSEP.

Nevertheless, the maser soon became operational in the Distant Early Warning (DEW) line. They were also used in receivers for transatlantic microwave communication via satellite. Their use in radioastronomy assisted in the discovery of the 3 K background radiation, remnant of the big bang, by Penzias and Wilson.

Our first laser (built with a home-grown maser crystal of potassium cobalt cyanide with chromium ions) was to be used to study interstellar hydrogen radiation with the Harvard radiotelescope. A piece of this crystal was to be inserted in the helium cryostat in the magnet gap in the background. This gives me an excuse to retell an old anecdote.

In 1959, Professor Charles Townes and I were corecipients of the Morris Liebmann Memorial Award of the Institute of Radio Engineers. Before the banquet, in the Waldorf Astoria in New York City, Mrs. Townes and Mrs. Bloembergen met in the ladies lounge and my wife complimented Frances

Townes on the pendant she was wearing. She told proudly how Charles Townes had given her a piece of ruby set in a gold frame to commemorate the success of his maser. He may have had a graduate student polishing the ruby, but he had personally soldered the gold frame. My wife was duly impressed, and back in our hotel room after the banquet she told me the story and popped the inevitable question, "When will I get a ruby?" I replied truthfully, "Well, dear, my maser works with cyanide."

Due to the work of Schawlow, Townes, Maiman, and others, it became clear that optical masers or lasers were going to be much more important than microwave masers. In 1960 the emphasis in my group at Harvard shifted from radio and microwaves to visible light, and in particular to the properties of matter at high light intensities. The field of nonlinear optics evolved. In the early 1960s the remarkable climate that existed for research support was such that we never had to submit a formal proposal to start doing optics research in the electronics program.

The backbone of continuous support of all my research work in nuclear magnetic resonance, in microwaves, and in nonlinear optics has come for a period of 37 years from the Joint Services Electronics Program. It was characterized by a minimum amount of administrative red tape, although this minimum is unfortunately no longer as low as it was in the 1950s and 1960s. There has always been complete freedom to choose new avenues of research and to publish results. I take this opportunity to thank JSEP for its support of nearly all of my scientific work over four decades, and I hope you will continue doing so for my colleagues.

Quantum Electronics at Columbia University

Dr. Charles J. Townes
University of California, Berkeley

It is a pleasure to be here, to participate in this celebration, and to see so many old timers and friends. We are here, of course, to celebrate the establishment of an institution, i.e. the Joint Services Electronics Program, and to recognize its importance and longevity.

I am reminded of the case of an English bastard. He was a real bastard, the illegitimate son of the Duke of Northumberland. This status weighed heavily on him, because he could never inherit any of the rights of his peerage, and the circumstances of his birth had a great deal to do with his subsequent actions. Publicly he said that he was going to do something to make his actions and his name remembered longer than the peerage of Northumberland; he left almost all of his money to found a new institution. His name was James Smithson and the institution was "an establishment for the increase and diffusion of knowledge among the people of the United States." He had never been to this country, but he decided that that was the thing to do for the future. I think we all know, from the reputation and accomplishments of the Smithsonian Institution, that he was right!

The foundation of institutions, particularly in growth areas, and the ideas that frame their philosophy, can be of tremendous importance to all of us. We are here to honor those who created these ideas, put them together, and made the wonderful and effective institution called JSEP.

I was asked to speak on behalf of the Columbia Radiation Laboratory. There are many things that might be said about that laboratory, and the great figures that have been

associated with it--Rabi, Lamb, Kusch, Schawlow, and many others. However, the organizers of this symposium asked me to talk about quantum electronics at Columbia University; so rather than present a general history and background of the Radiation Laboratory, I will dwell on a specific example to illustrate the kind of things that have been important in JSEP support of that laboratory.

Quantum electronics has its origins in the interplay, back and forth, between different disciplines. I often have thought that the origins of the maser and the laser might well have come much earlier than it did, because I know of no individual theoretical concept that was new in the evolution of those devices. (There were certainly many forerunner investigations that preceded the advent of those devices.) What I think really delayed the development of quantum electronics was a lack of the piecing together of ideas from a variety of fields.

The interaction between technology and science is not a one-way street from basic to applied work, but flows in both directions, from technology to science, and science to technology. The field of quantum electronics might be said to have begun in the "K" band radar development of World War II. I was somewhat involved, in that I was very much concerned about the possibility of absorption of the 1.25-centimeter radiation by water vapor--a concern shared by some others. In looking into that question carefully, I recognized that while it might, and did, deny practical use of radar based on that wavelength, nevertheless it could open up a fascinating field of science, the field of microwave spectroscopy. It was out of this field that quantum

electronics really grew. One might suppose that it was just a matter of chance that someone in the field of microwave spectroscopy initiated quantum electronics, but evidently, it was something more than that. There were three apparently independent ideas connected with quantum electronic device origins; one was generated at the Radiation Laboratory of Columbia, the other at the University of Maryland, and the third in the Soviet Union. These ideas were somewhat different in completeness and in timing. However, they apparently were quite independent and all were produced by people working in microwave spectroscopy--three independent starts.

I, myself, initially worked in microwave spectroscopy research at the Bell Telephone Laboratories, which, along with RCA, General Electric, and Westinghouse, had programs in that newly emerging field. (Note that much of that field of investigation was, indeed, initiated in industrial laboratories; that is where the surplus military microwave equipment was.) Now one might think in retrospect, "that is just great, these industrial giants surely recognized that what was going on in microwave spectroscopy might be important to their future." But, in fact, one cannot quite say that about industry. A friend of mine in one of these laboratories was told by his superiors that "the science is fine, but we want you to start measuring the dielectric constants of solids; that is what is important to us." One of the other companies just gradually let the program die, and a third company was sufficiently disinterested to suggest that maybe the gentleman doing that work might like to go to a university, and he did. Bell Laboratories, where I had sufficient support all right, was not, however,

interested in extending the field; this, despite the fact that Bell Laboratories is a far-sighted place, and certainly one of the best. But the Columbia Radiation Laboratory already had a start in the field; it had equipment from its World War II research, particularly K-band gear, covering the region of the electromagnetic spectrum that was exciting at that time. When I was invited to come to Columbia with other people who were very much interested in microwave studies, I accepted because of the equipment and the people and the interest there. JSEP was backing a liberal program for general and open-minded support of research in the microwave field. I do not think one should criticize industry for dropping research in that field; it is indeed difficult to predict the future payoff of research. What is important is that universities have a special role in carrying out fundamental experimentation, experimentation that sometimes leads to new industry but which sometimes does not. In time, industry played its own role, in that it came very strongly into quantum electronics in the long run. Government has its role too; interaction among these (i.e. universities, government, and industry) really is what we are discussing.

The government played part of its role in the evolution of devices based on quantum electronic principles when I, new to the Columbia Radiation Laboratory, was asked by the Navy to head a committee whose purpose was to examine the possibility of devising new techniques for operating in the millimeter range of the radio spectrum. Moses Long of ONR was sort of the activator. I was interested because it was clear that was going to be a rich field for new, high resolution spectroscopy, an extension of my research in

microwave spectroscopy. The committee brought together people in a variety of fields that might have some bearing on this effort. Among these were Marvin Chodorow from Stanford, well-known in microwave technology; John Strong, a very well known infrared experimentalist of the time; John Pierce, of BTL, a great expert in electron tubes; and some others. We reviewed the Navy program, trying to develop ideas and suggestions for new things that might generate very high frequency waves. Many interesting ideas were discussed at these meetings. I was working on a variety of things that seemed promising, such as Cerenkov radiation from the surface of dielectric materials, and magnetron harmonics. These techniques worked somewhat, but none of them terribly well. It was not in New York, as someone suggested, but rather in Washington, just before a committee meeting, when the key to getting at the millimeter waves came to me. I was mulling over our lack of real progress, while sitting on a bench in the early morning admiring the azaleas of Franklin Park. I just did not see how one could make the necessary resonators and build the very fine structures with necessary precision, and dissipate the necessary power in them. I had been toying with the possibility of using molecules or atoms as resonators. Clearly, for very high frequency that was the only way to do it, but how? Then I suddenly recognized the possibility. Perhaps the work done at Harvard by Purcell and Ramsey on inversion of populations was in the back of my mind. In any case, I realized there was a way of getting amplification out of molecules and atoms, by avoiding thermodynamic equilibrium. I also remembered a recent talk of the German physicist Paul, who developed a beam technique for obtaining large numbers of molecules or atoms in a single state. On the

back of an envelope I quickly worked out a possible way of obtaining an oscillator at high frequency. It seemed difficult, and it was a few months before I could really get anything started on it. Fortunately, there was a good student, Jim Gordon, who was willing to try it. He was joined by Herb Zeiger, a young post-doc who had been working with Rabi and had experience in molecular beam techniques. Although the principle seemed clear, the problem was whether we would be able to get over the margin of oscillation.

The importance of molecular and atomic phenomenon for radio device technology was not an entirely new thought to me. I had, while at Bell Laboratories, written a memo saying that, in the long run, circuit elements at very high frequencies were likely to be using molecular or atomic resonances. But at that time I had not foreseen the possibility of amplification by stimulated emission. Rabi also had pointed out that for very constant frequencies, atomic resonances offered promise. I already had been active in the use of molecular resonances for frequency control, because the Army Signal Corps was interested in those techniques. In that regard, one should note that both Zacharias's cesium atomic clock and the extremely precise hydrogen maser device developed by Prof. Ramsey at Harvard, are products of JSEP sponsorship.

We got our first system oscillating after 3 years of work, which is the normal time constant for a graduate student career thesis, and hence the way things tend to move in a university. While we were building it, there was not great excitement about the idea. People said it was a nice idea, but nobody tried to copy it and many were skeptical.

From the beginning, the maser was obviously a good oscillator. I also recognized, at that time, that it also would be a good amplifier; at least it was more sensitive than any previous one, though not very tunable. However, progress took another turn because about that time I took a sabbatical, working in Paris with one of my previous graduate students. In Paris, he and a French physicist had been measuring the relaxation time of certain paramagnetic ions in solids. I found to my surprise that those relaxation times could be very long, and recognized that one hence had the possibility of making an amplifier that was easily tunable and had a much wider bandwidth than the initial maser. We worked on that some in Paris before I moved on to Tokyo, and in the middle of the year made a trip to the Bell Laboratories to talk to the people there about the new device. Note the trail of interaction between different laboratories and people in different disciplines, because that was a very important part of the process. A little later, stimulated by independent work of Strandberg and ours in Paris, Bloembergen had his substantially better idea of a three-level paramagnetic system. By that time I was in Japan, working with one of my previous associates from the Columbia Radiation Laboratory. We wanted very much to understand the noise aspect of the maser. It was easy to show that such amplifiers could get down to the fundamental noise limit of one quantum per unit bandwidth, but the details of the statistics were not so clear. Also in Tokyo, I ran into another friend of mine from Columbia who was a biologist. In discussions with him, he brought up a solution to the problem of population changes in microorganisms, which sounded relevant to photon numbers in a maser. I looked up this interesting paper and applied its approach,

in somewhat modified form, to noise in a maser amplifier. So, by the time I came back to the United States from Japan I felt we understood the statistics of the noise in the maser amplifiers, including particularly the promising paramagnetic versions.

As soon as I got back to Columbia we devoted our efforts to making a good maser amplifier for radio astronomy. I was interested in radioastronomy; and as Arnold Shostak indicated, the first use of the maser amplifier involved our work with NRL, for radioastronomy observations using their early radio telescope. It was Kikuchi, a Japanese-American working in Michigan, who first proposed ruby as a good maser substance. This is what we used, and it has been the standard material ever since for maser amplifiers. Subsequently, Arnold Penzias, another Columbia student, built another one to look for hydrogen lines. It was later, at Bell Labs, that Penzias and Wilson used a maser in making the important discovery of big-bang radiation.

One might wonder where was the laser all of this time? We were working hard on masers, in the microwave region. In a way, we were distracted because the program was so exciting, and we were enjoying the many new facets of the new high quality oscillators and sensitive amplifiers. Many physicists and engineers were stimulated to build, or suggest new maser devices. It may seem obvious that somebody had to be working on lasers during that time. My own original idea about masers was for getting down into the far infrared; the first one I had written up was to operate at half a millimeter. However, we built the first one in the microwave range, because the microwave techniques were

available and easier. I thought to myself that of course we would get down to the submillimeter range or in the far infrared sometime, but it would be a bit more difficult. Sometime during this period--I suppose it was probably late 1956 or early 1957--Bill Otting, of the Air Force Office of Scientific Research, dropped in to see me and asked whether I would be willing to write a review paper or discussion about the possibility of getting into the infrared. That, I think, showed a great deal of prescience on his part. It was typical, at the time, of our relations with military scientists. They would come by and want us to do this or that. Sometimes we did it, sometimes we did not, which they understood. In this particular case, I told Otting I thought it was very interesting and I wanted to do it, but I really did not have any great new ideas over the original one and hence did not feel like there was anything much to write at that point. I did say that when I had additional good ideas I would consider writing such a paper. I also mentioned that there was a very bright young man who had just gotten his Ph.D., namely, Ali Javan, who might be willing to do a write-up on the possibilities. Well, Ali was busy with other things too. He was not inclined to do so at the moment. I believe the first time that anything was written about really getting into the short infrared came from an Air Force study in the summer of 1957. That study was on the future of the Air Force, and what they should be working on. In that report one will find mention of the possibility of pushing masers perhaps as far as the mid-infrared. I was part of that study, and of course, that was the reason it was in there. The report was never issued because it was written in the late summer of 1957. Sputnik came out in October, and the report was outdated by

that event. The Air Force had carefully avoided talking about space work in the report because that was unpopular in Congress until after Sputnik, so that report never came out. However, it does exist in the files of the Air Force. In September of 1957 I decided that I really ought to sit down and think about how to get to shorter wavelengths--meaning pushing on down well below the millimeter wavelengths. A little thought convinced me that the gain was just as easy to get in the optical region as in the far-infrared domain. Equations showed that you typically did not need any more atoms in the optical than in the microwave region, and that the gain was just as easy to get in the optical region. Since we knew a lot more about the optical region and optical techniques, I decided we should just jump directly into the optical region rather than initiate work in the far infrared. But one of the real problems in doing that was to design a single-mode resonator. I designed a resonator with some big holes in it that suppressed some of the modes. I was going to bounce some of the exciting radiation back and forth in it and get rid of some undesired modes, but I recognized that there were still many modes present and that radiation would probably hop from one mode to another. Nonetheless, it would be an interesting development to obtain an optical or infrared oscillator. Since at the time I was at Columbia University, one might wonder then, why wasn't the laser a direct JSEP Columbia University invention? The reason was somewhat accidental. I had done the above described work at my desk. But I was consulting once every two weeks, for a day, at the Bell Telephone Laboratories. I went out to the Bell Telephone Laboratories and talked with Dr. Arthur Schawlow, who had done his post doctoral work with me at

Columbia, and had then gone to Bell Labs. He said he was very much interested in the idea and had been thinking about such things. As I told him about what I had done, he asked about a Fabry-Perot resonator, since that would eliminate a lot of the modes. Immediately I recognized that was the real idea for a resonator. We worked out ways in which one could eliminate all but one mode in a Fabry-Perot system. I then had the problem of what was the ethical thing to do--how does one divide the pie here? Was I working for Columbia and JSEP, or was I working for Bell Labs when I was sitting in my office? I recognized that if both institutions were involved it was going to get complicated. Well, I reasoned, Art Schawlow is there at BTL, and he has made a big contribution to the subject; so at least half of it should be a Bell Laboratories invention. So making it a Bell Laboratories patent was a natural way out. However, I think from the point of view of ideas, JSEP has equal claim. It was a completely arbitrary decision on my part to say, "Well, let's just call this a Bell Laboratory device--that is the simplest thing to do." But JSEP was a crucial contributor to the development, including Art Schawlow's and my association at Columbia. I do not think one should make a great deal about the details of what happened at which institution, because the interaction between institutions was a very important part of the process, and the ease of naturalness of that interaction is something we must try to preserve.

Sometime in 1958, we started working on trying to build a laser at Columbia. A new graduate student, Isaac Abella, was going to undertake this as a project. He was working with us on that project as a thesis problem. However, I

got persuaded that it was more important for me to be down in Washington for a couple of years, so in the fall of 1959 I went down to the Capital. Isaac was still working at it at that time and surely would have made a laser eventually. But now was the time for industry to shine. Industry already had become excited about masers as amplifiers, particularly the three-level, solid-state maser. There also had been industrial interest in the ammonia maser as a constant-frequency oscillator. Students in microwave and radio astronomy had been hired by industrial groups to work in that field. In fact, the maser business became so exciting that the editor of *Physical Review* stated that too many people were submitting papers on those devices and he felt forced to put a limit on publication on that topic. But what really ticked things off as far as the lasers were concerned, was the paper by Schawlow and myself. Everybody was ready by then to recognize their excitement and potentiality. Industry got going, and all of the first types of lasers were first made in industry. Almost all of the important actors in quantum electronics came out of the field of radio and microwave spectroscopy developed in the universities. The very first laser was that of Ted Maiman. (Maiman was grandson of the Columbia Radiation Laboratory program, because he was a student of Willis Lamb's who had been a major figure there.) Quantum electronic techniques are really applied spectroscopy. Maiman was working at Hughes Research Laboratories, having been brought there by Harold Lyons. Lyons had been involved in the atomic clock business at the National Bureau of Standards and was closely connected both with Zacharias's work and with our ammonia work. The next laser that was built is not so widely known simply because it has not been a commercial product.

It was made at IBM by Peter Sorokin and Mirek Stevenson. Sorokin was one of Bloembergen's students, from the Harvard program. Stevenson was one of my students from the Columbia program. Both had been hired by IBM. Together they made the second and third lasers. Those were crystal lasers, interesting lasers at the time but they have not been commercially useful. The next laser was made by Javan, Bennet, and Herriot. Javan had gone from Columbia to the Bell Laboratories. This important device was the helium-neon laser--the most popular laser used today. (It has sold more than any other laser and has been an exceedingly useful laboratory, as well as industrial tool.) Both Javan and Bennett had come from our Columbia program. Herriot was an optical physicist who had been at Bell Laboratories for some time. Javan provided the initial idea and figured out the atomic physics involved in collaboration with Bennett, while Herriot had concentrated more on the optics. Together, the three of them got the first gas system going.

Today, one may note that lasers can be made out of almost anything. Almost anything can be made to amplify, if properly excited. I must say in the early days that would have been surprising. I thought it remarkable that, although spectroscopy had been going on for generations with very highly qualified people working in the field, and a wide variety of experiments had been carried on with gas discharges, that no one had ever seen any kind of amplification. Therefore, I reasoned, the necessary conditions must be pretty tricky. By now, many different varieties have been generated by clever people and they do not seem difficult.

Even after successful lasers were made, it still took the field a while to get going, in an industrial sense. There was a famous quip around which people used to twit me, that the laser was "a solution looking for a problem." Nevertheless, industry was steadily very active in the field. It seemed an exciting one, and by the late 1960s it began to really see use and find very significant applications. By now there are, of course, an enormous number of important laser uses. The last survey I saw stated that it was about a 4.5 billion dollar industry. After all, such a device that combines the fields of optics and electronics must eventually touch a wide variety of scientific and technological areas. The many uses of the laser are now well known. I want particularly to call your attention to this almost textbook example of basic research and its contribution to technology, industry, and the military. One might also add the interaction back on the basic research of the now many industrial and technological developments in this field. Few lasers are built by universities these days; the commercial devices do much better than we can do at the universities.

But in addition to this interaction, back and forth, I want to call your attention to the problem of planning long-range research with applications in mind. That is a very delicate and difficult problem. We must keep reminding ourselves, our Congressmen, and military leaders about its peculiar nature. How, for example, would one have planned to produce a brighter light back in the 1950s? He probably would have gone to an industrial laboratory, perhaps Westinghouse or General Electric, and asked, "Can you give us a

brighter light?" And they would have done a good job and produced a light of intensity increased by a factor of 2, or something like that. But the laser has increased light intensity by a factor of about a million or more. Alternatively, suppose someone had asked, "How can we do research to produce a new cutting and welding tool and a new tool for hardening the surface of metals?" Probably the answer would be to go to a metallurgist, or an arc welder perhaps, and asked them to do something, and they might have improved the techniques a bit. Certainly no one would have gone to a physicist and said, "Would you please study the interaction between microwaves and molecules, because we want a brighter light or new metal-working techniques." How would you plan to produce a better amplifier? Obviously, the answer would have been to go to electron-tube people and they would have again done a good job, and maybe improved things by a factor or 2. But with the maser, suddenly there was an increase by a factor of 100 in sensitivity over previously known amplifiers. What about surgical tools, or what about communications? How would you improve communications? How would you attain wider bandwidths and cheaper communications? Again, you would go to the communications industry and they would work on it and do something, certainly. However, the tremendous increase in bandwidth available from the laser would not have occurred. This field, which grew out of the rather esoteric study of the interaction between microwaves and molecules, has effectively revolutionized every one of these fields. How could we have possibly planned for it?

I believe the plan which JSEP has developed is the right and only one; namely, to support people who are

interested in new things, working in fields that are producing new ideas and that have some bearing on practical usage, and to give such people the freedom to innovate, with the kind of flexibility and chance to develop their own ideas that can pay off. Obviously, directed research is necessary for certain purposes--one must not down play that. Nevertheless, the nation's scientific effort needs to be looking forward in a way that is simply not predictable in detail. Time after time, we can see from history that what is quite predictable is that basic research and a good program of science will lead us in a direction of great value to the country, and that we must nurture an effective program of basic research as best we can.

RECENT JOINT SERVICES ELECTRONICS PROGRAM ACTIVITIES

Chaired by

Dr. Horst R. Wittmann

Air Force Office of Scientific Research

Technical Coordinating Committee, JSEP

*Some Joint Services Electronics Program Activities
at the University of California, Berkeley*

Dr. John Whinnery
University Professor
University of California, Berkeley

I appreciate very much the opportunity to take part in this birthday celebration and to tell you about some of the features we have found so important in the Joint Services Electronics Program. We at Berkeley joined JSEP in 1961, through the work of Sam Silver and Don Pederson of our university, and many supporters at AFOSR, ONR, and ARO. The initial program was funded by combining DOD programs already in existence in our department. But even before that we had the influence of results from JSEP research at other schools. This morning we heard about the birth of quantum electronics (QE) through JSEP support at Columbia and Harvard. Our first QE thesis was that of Amnon Yariv, on "F" Center Masers, clearly building on that work. Another example is that of the beautiful JSEP-supported noise reduction work of Dean Watkins at Stanford. This certainly influenced my own work with M.R. Currie in microwave-tube noise at Berkeley, ultimately leading to his very-low-noise traveling wave tubes, using the Currie gun, at Hughes. In addition, the Technical Advisory Committee meetings at the various JSEP schools were major sources of scientific and technical interchange for that period.

In selecting examples from our program, I have chosen five projects that will give you some idea of the variety of the projects as well as to show how some apparently unrelated projects have supported each other. The first of these is the JSEP role in our important integrated circuits and computer-aided design work.

Shortly before our entry into the JSEP program, Don Pederson had seen the necessity of building a facility for the fabrication of semiconductor devices and circuits. At

that time the conventional wisdom was that universities could not afford such facilities, but should concentrate on theory and leave the fabrication to industry. Don realized that this would lead to a very sterile program and that, on the contrary, universities could not afford to be left out of this key development in electronic circuits. The first university IC lab was started with support and encouragement of the Air Force Avionics Laboratory, and the support was continued by JSEP when we joined that program. In particular, a critical JSEP equipment grant in 1962 established the unit as a working lab.

The first IC lab was rather primitive by modern standards, but helped us accomplish a lot of good research and turned out graduates who have become leaders in the IC industry. About 5 years ago we started a modernization program with basic support from the State of California, but with help from industry and again a key equipment grant from JSEP.

Integrally related to the fabrication facility was the computer modeling, which led to powerful CAD software, such as SPICE. Figure 1 shows some of the early history. JSEP-supported BIAS was the key program in moving from existing programs, such as SCEPTRE, to SPICE and its derivatives. The advantages were in simplicity, portability, and the use of better device models. All this was motivated by the necessity to explain the performance of the real circuits made in our IC lab. SPICE is now the dominant IC analysis and design program, at last count used in more than 4,000 installations around the world, including many DOD facilities. A huge CAD effort in our department has grown

FIGURE 1
IC HISTORY AT THE
UNIVERSITY OF CALIFORNIA, BERKELEY

THE BEGINNINGS	1960
LAB OPERATION	1962
FIRST CAD NEEDS	1966
BIAS	1969
CANCER, SLIC, SINC	1970
SPICE	1972
LAYOUT TOOLS	1980
CAD/CAM CENTER	1982

(See Figure 2), including current JSEP-supported work by Professors Kuh, Chua, and Sangiovanni-Vincintelli.

The point of the JSEP support for this work was that it was given at a critical, early stage, both for the facility and the computer-aided design work, before it was obvious to everyone that this should be done. And it was also critical that the CAD development was integrally related to the fabrication of real devices and circuits, and grew out of the need to understand these physical entities.

The next example of a very influential program, begun with JSEP support, is a systems example. This is an interactive graphics and retrieval system with the acronym INGRES (Figure 3). This major database management system was begun in 1973 by Professors Michael Stonebraker and Eugene Wong, originally for the PDP 11/70 Unix system. The Sloan Foundation provided some of the computers, and ARO gave key support, but the original research was carried out under JSEP auspices.

The objectives of the project are shown in Figure 4. The advantages of relational database systems over the hierarchical systems in use at that time had been discussed, but up to that point had been impractical to implement. The figure illustrates the issues needing research and the successful goals achieved. Figure 5 shows in more detail what is meant by a relational system. Its successful achievement was by ingenious programming using higher level languages.

FIGURE 2
CAD/CAM IN EECS/UCB

TOPICS	PROJECTS	FACULTY
PHYSICAL DESIGN	24	6 (KUH, NEWTON, OUSTERBOUT, SANGIOVANNI, SEQUIN, THOMPSON)
SIMULATION AND VERIFICATION	24	5 (CHUA, NEWTON, PEDERSON, SANGIOVANNI, SEQUIN)
MODELING	11	6 (CHUA, HODGES, HU, KO, NEWTON, PEDERSON)
CAM/ME	8	5 (GLASSEY, HODGES, NEUREUTHER, OLDHAM, ROWE)
DATABASE AND SYNTHESIS	11	7 (GRAY, KATZ, NEWTON, OUSTERBOUT, SABIN, SANGIOVANNI, TURIN)
DESIGN METHODS	26	8 (BRODERSEN, GRAY, KATZ, NEWTON, POLAK, SANGIOVANNI, SASTRY, SEQUIN)
TOTAL	115	

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PROCEEDINGS OF THE ANNIVERSARY SYMPOSIUM (40TH) OF THE
JOINT SERVICES ELE. (U) BATTELLE COLUMBUS LABS RESEARCH
TRIANGLE PARK NC D ROBB ET AL. JAN 87 AFOSR-TR-87-1497

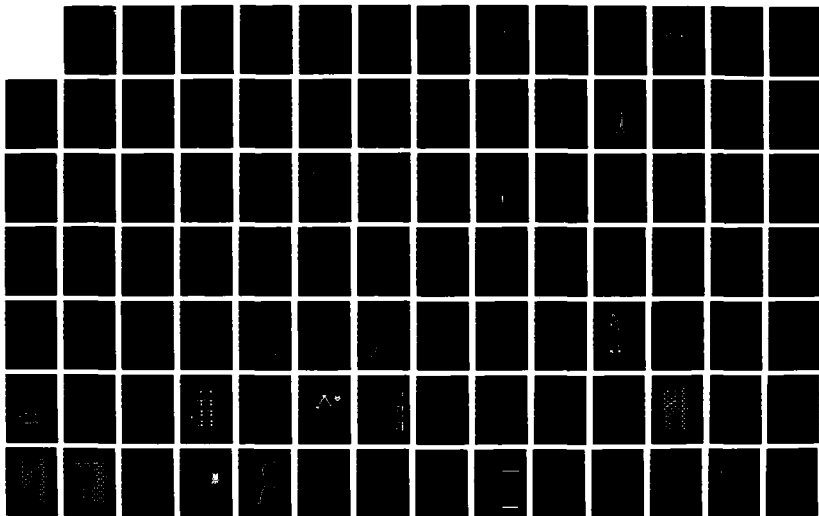
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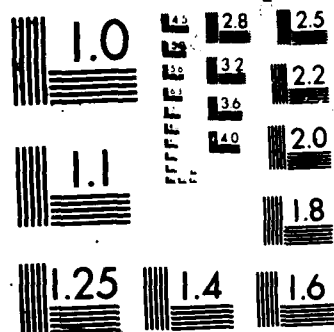


FIGURE 3
INTERACTIVE GRAPHICS AND RETRIEVAL SYSTEM
(INGRES)

- o A MAJOR DATABASE MANAGEMENT SYSTEM**
- o BEGUN IN 1973 BY MICHAEL STONEBRAKER AND EUGENE WONG**
- o ORIGINAL ENVIRONMENT: PDP 11/70--UNIX**
- o EARLY SOURCES OF SUPPORT**
 - JSEP**
 - ARO**
 - SLOAN FOUNDATION**

FIGURE 4
OBJECTIVES

- o TO TEST THE THEN UNTRIED CONCEPT OF "RELATIONAL" DATABASE SYSTEMS
- o TO DO RESEARCH ON ISSUES IN DATABASE MANAGEMENT, E.G.,
 - QUERY OPTIMIZATION
 - CONCURRENCY CONTROL
 - RECOVERY
- o GOAL: TO DESIGN AND IMPLEMENT A COMPLETE AND USABLE SYSTEM, NOT MERELY A PROTOTYPE
- o THE FIRST DELIVERABLE RELATIONAL DATABASE SYSTEM
 - AVAILABLE FREE TO UNIX LICENSEES SINCE EARLY 1976

FIGURE 5
WHAT'S RELATIONAL?

- o SIMPLE USER VIEW**
 - DATA SEEN AS TABLES, NO OTHER STRUCTURES**
- o DATA INDEPENDENCE**
 - CLEAN SEPARATION BETWEEN "USER VIEW" AND "STORED REPRESENTATION"**
- o POWERFUL DATA MANIPULATION**
 - NEW TABLES CREATED FROM OLD**
 - DATA NOT MERELY ACCESSED**
- o NONPROCEDURAL QUERY LANGUAGE**
 - USER SPECIFIES WHAT IS WANTED, NOT HOW TO GET IT**

Figure 6 shows the current status of INGRES. It is in nearly universal use with widespread commercial distribution. Users include 90 DOD and 315 government sites. Again, the early support of JSEP was critical to this development.

The subject of electromagnetics, with emphasis on antennas, scattering, and microwave networks, has always been a strong part of our program from the time of Sam Silver and Vic Rumsey to the present. It has been a program with strong ties between theory and experiment. The theoretical approach in recent years has been that of utilizing the increasing power of digital computers, combined with powerful analytical tools, to solve programs that could not be done by formal analysis alone. Figure 7 shows some of the important problems and approaches carried out by Professors Ken Mei and Dodge Angelakos, under JSEP sponsorship. There has been an increasing sophistication in the tools, finally arriving at the ideal we have dreamed of for years--the solution of EM boundary-value problems of arbitrary shape in three dimensions, in time-varying form. These powerful techniques are now being applied to the computer-aided design of microwave and millimeter-wave integrated circuits.

In selecting one item from the list of EM accomplishments, I would like to show some examples of the scattering calculations of dielectric bodies embedded in a lossy dielectric medium. This problem, using numerical calculations based upon the generalization of the Sommerfeld integral to include the effect of multipoles, has obvious application to the important problem of detecting land mines by electromagnetic means. Figure 8 shows the calculated scattering pattern from a flush-buried dielectric cylinder,

FIGURE 6
CURRENT STATUS

- o BASIS FOR SEVERAL COMMERCIAL PRODUCTS (RTI-INGRES, CA-UNIVERSE, ETC)**
- o VERSIONS NOW AVAILABLE ON ALL MAJOR COMPUTERS FROM PC TO LARGE MAINFRAMES**
- o ACTIVELY USED AT SEVERAL THOUSAND SITES**
 - MOST UNIVERSITY UNIX SITES**
 - MAJOR APPLICATIONS AT GM, GE, AT&T, SCHLUMBERGER, ETC.**
- o ENJOYS 45% SHARE OF VAX/RELATIONAL MARKET**
- o 315 U.S. GOVERNMENT SITES**
- o 90 DOD SITES**

FIGURE 7

JSEP-SUPPORTED ELECTROMAGNETICS RESEARCH

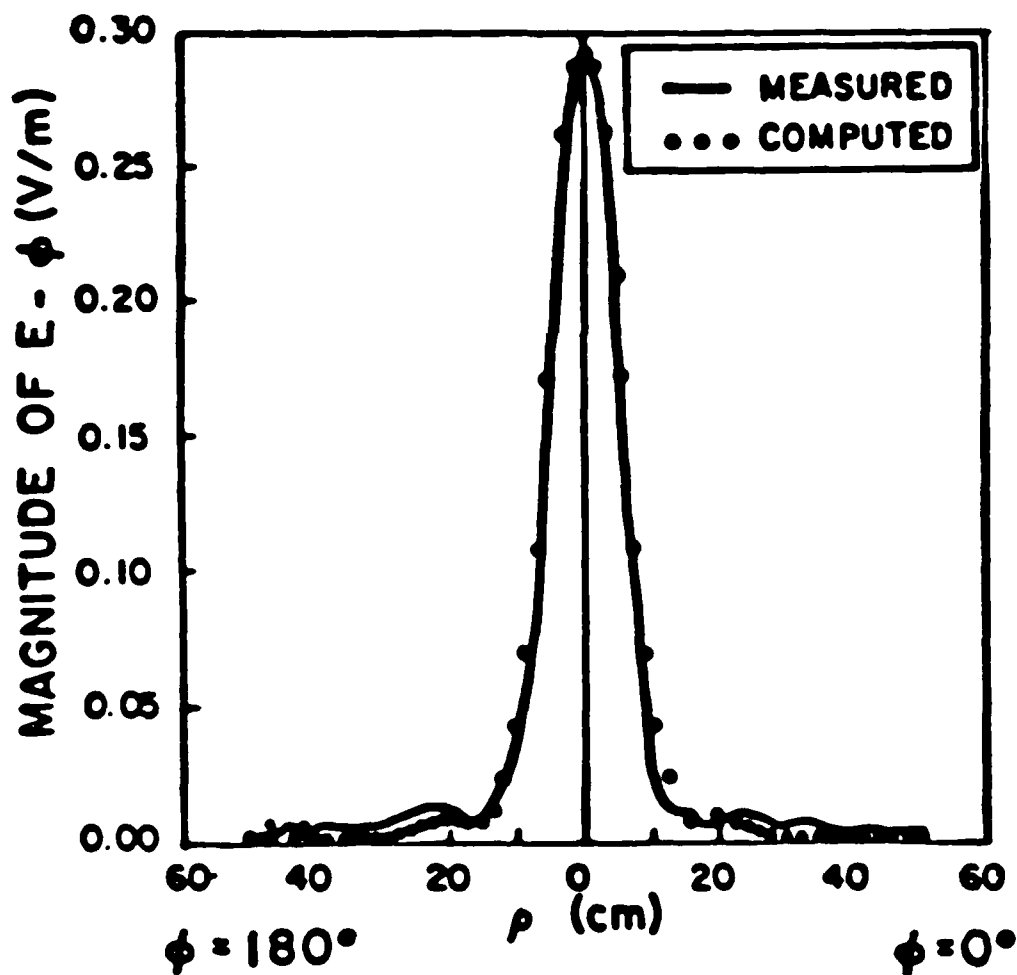
K.K. MEI AND D.J. ANGELAKOS

1965	CALCULATION AND MEASUREMENT OF CURRENTS ON SPIRAL ANTENNA
1970	ANTENNAS IN PLASMAS
1975	FINITE-ELEMENT METHOD APPLIED TO INCLUDE MULTIPOLES--SCATTERING FROM DIELECTRIC BODIES AND MULTIPLE SCATTERS
1980	GENERALIZATION OF SOMMERFELD'S INTEGRAL--SCATTERING FROM BURIED TARGETS
1980-85	TIME-DOMAIN FINITE-ELEMENT TECHNIQUES
PRESENT	CAD OF MICROWAVE-INTEGRATED CIRCUITS

FIGURE 8

COMPUTED AND MEASURED MAGNITUDES OF THE PERTURBATION FIELD OF THE DIELECTRIC CYLINDER FLUSH BURIED IN A LOSSY GROUND FOR A FREQUENCY OF 700 MHz.

PERTURBATION FIELD = 1" (2.54 cm) ABOVE THE INTERFACE



and the comparison with measured results from our field station scattering range. Figure 9 shows phase. Here there is an error in phase measurement because of an imperfect probe connection. Figure 10 shows 3-D and contour plots for a completely buried cylinder irradiated at an oblique angle. The preceding had relevance to the problem of detecting anti-personnel land mines. Figure 11, the last of the series, is that of scattering from a partially buried sphere, and is related to the problem of detecting anti-tank land mines from aircraft. It is known that one can detect such obstacles with electromagnetic waves but that selection of power levels and frequency are important; this work gives a basis for quantitative analysis. But I would like to stress again the power of the latest methods--allowing time-domain field calculations for three-dimensional problems. Advances in computers and computational techniques, combined with powerful analytical techniques, made this result possible.

Superconducting devices using the Josephson effect have tremendous potential, but also some limitations. It is clear that they will continue to have important special purpose applications, as in the SQUID (superconductor quantum interference device) magnetometer. Their role in large-scale computers is still uncertain, but it is clear that research to determine the potential must continue. Certainly Japanese researchers recognize this and have very active programs in the subject.

JSEP support of Professor Ted Van Duzer's work in this subject is summarized in Figure 12. Clearly the support has been critical in establishing this program, and several

FIGURE 9

COMPUTED AND MEASURED PHASES OF THE PERTURBATION FIELD OF THE DIELECTRIC CYLINDER FLUSH BURIED IN A LOSSY GROUND FOR A FREQUENCY OF 700 MHz. (PERTURBATION FIELD = 1" [2.54 cm] ABOVE THE INTERFACE.)

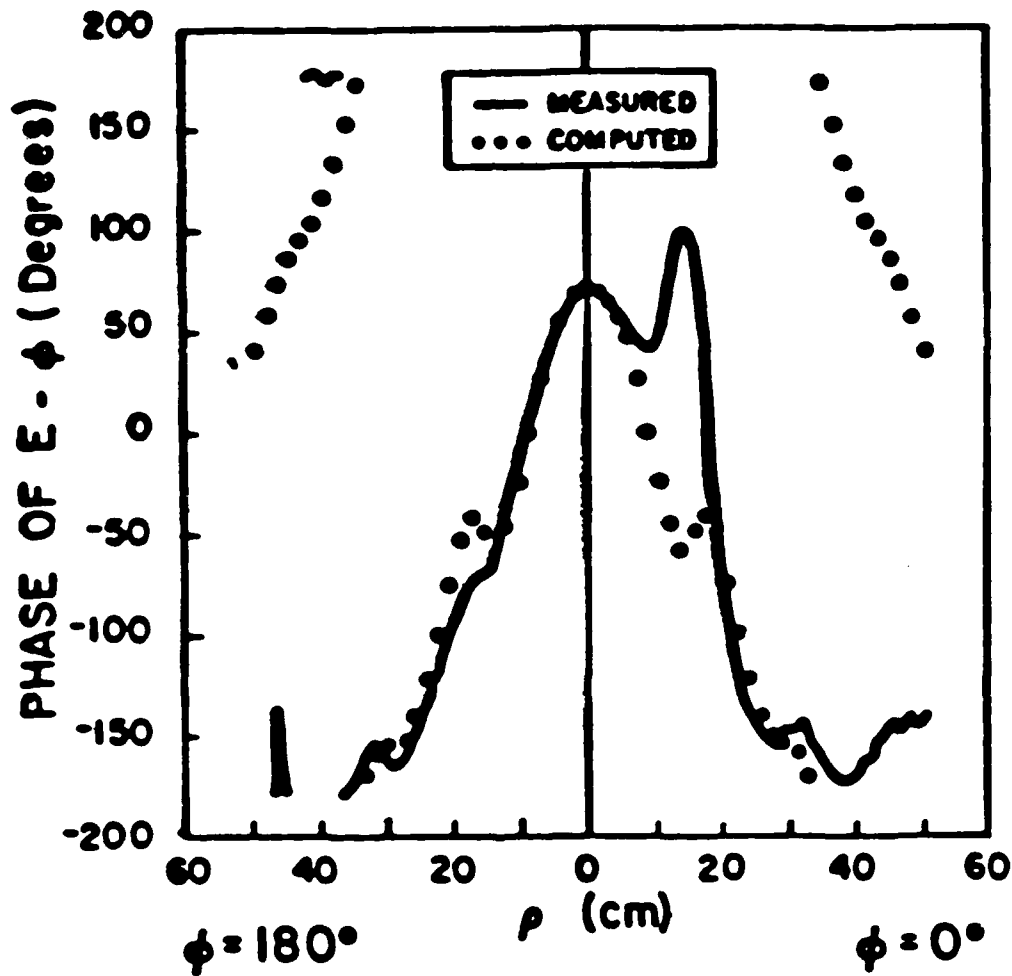


FIGURE 10

3-D AND CONTOUR PLOTS AS SCATTERED E-FIELD AMPLITUDE ON THE
EARTH SURFACE.
($\theta_I = 45^\circ$, FREQUENCY = 700 MHz)

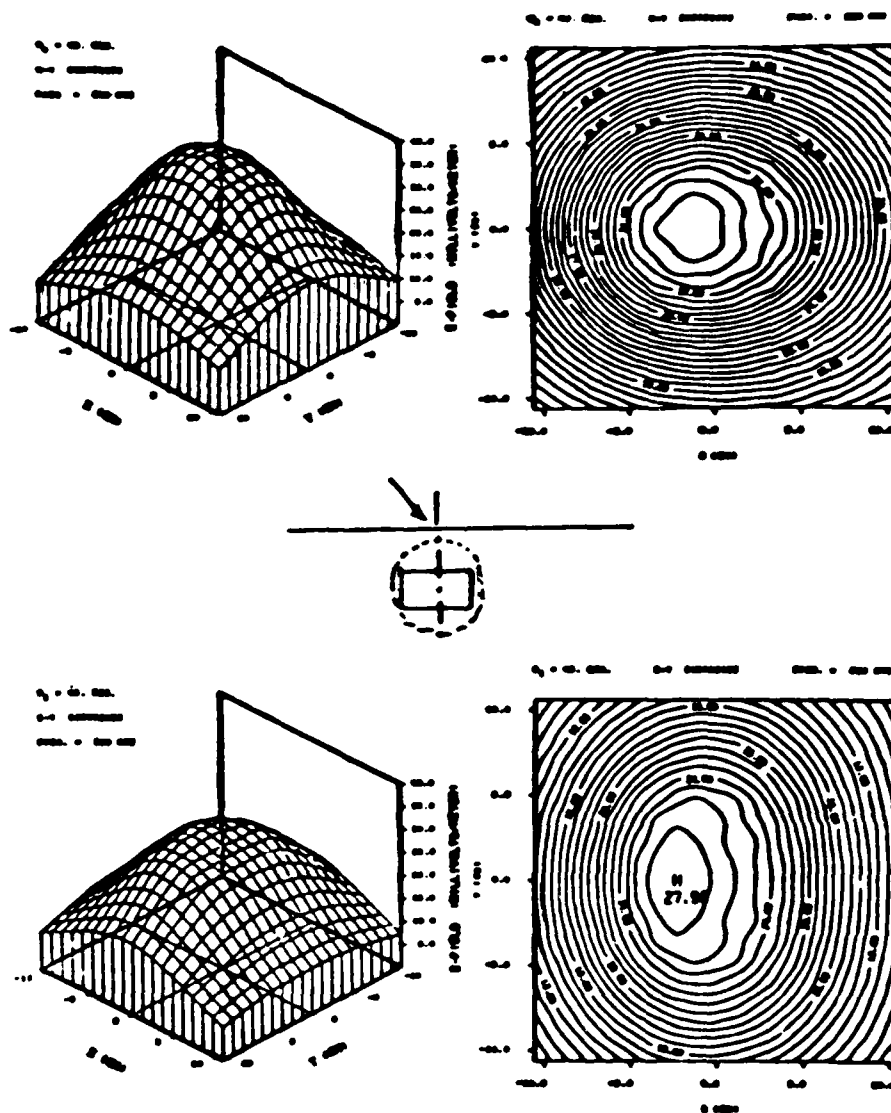


FIGURE 11
FAR FIELD OF SCATTERING BY PARTLY BURIED TARGET

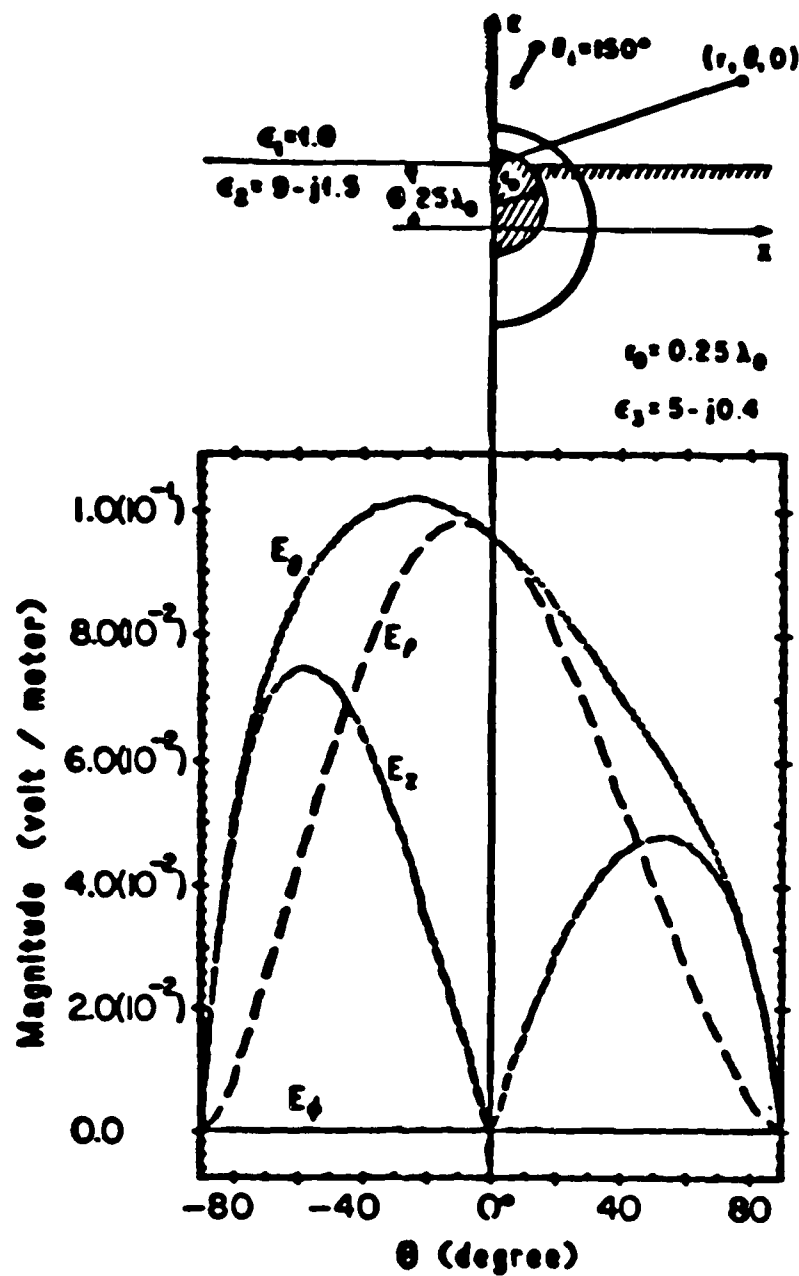


FIGURE 12

SUMMARY OF JSEP SUPPORT FOR SUPERCONDUCTOR ELECTRONICS

ELECTRONICS RESEARCH LABORATORY, UNIVERSITY OF CALIFORNIA,
BERKELEY

- o MODEL FOR Su-SEMI-Su JOSEPHSON JUNCTIONS (1972)
(ACCEPTED MODEL SINCE THEN)
- o DEMONSTRATION OF Pb-Te-Pb JOSEPHSON JUNCTIONS
- o FORMATION OF JOSEPHSON JUNCTIONS WITH CRYSTALLINE
SILICON MEMBRANES AS BARRIERS (COMBINED WITH ARMY
SUPPORT)
- o MODEL FOR SILICON ANISOTROPIC ETCHING; DEPENDENCE ON
DOPING
- o OTHER CONFIGURATIONS OF JOSEPHSON JUNCTIONS COUPLED BY
CRYSTALLINE SILICON
- o SUPERCONDUCTOR-INSULATOR-SUPERCONDUCTOR TUNNEL-JUNCTION
DETECTOR WITH THIN-FILM V-ANTENNA
- o TECHNIQUE FOR ACCURATE FABRICATION OF TUNNEL-JUNCTIONS
FOR SUPERCONDUCTIVE INTEGRATED CIRCUITS
- o MINIATURIZATION/OPTIMIZATION OF JOSEPHSON LOGIC CIRCUITS

of the achievements are now accepted models or procedures in this field. I would like to describe in a bit more detail the current work on miniaturization of Josephson logic circuits.

Figure 13 shows the advantage in speed and number of gates per chip in moving from a common 5- μ m feature size to 1- or 1/2- μ m size. Figure 14 shows the constraints on critical current (maximum and minimum) on critical current density and on inductance for stable, controlled switching. Figure 15 shows the chip layout. Delay will be measured on a single gate with an on-chip superconductive sampler, and in a string of N gates with fast sampling scope. Figure 16 shows a sketch of the measurement setup.

This project, incidentally, is another that could not have been done without the facilities of our modern micro-fabrication laboratory.

JSEP support has also been critical to our quantum and optical electronics program. Each of the faculty members in this area have had several projects with JSEP sponsorship. I will not attempt to list all of them, but in Figure 17 I have selected one subject for each by way of example. Many of the projects were done by two or more faculty members working together, as the figure indicates, but it would become too complicated to show all these interactions. Also listed are some of the students who worked on these projects. Those of you who know this field will recognize that all are still active in the field, and many are leaders.

FIGURE 13
CIL
(INTERFEROMETER TYPE)

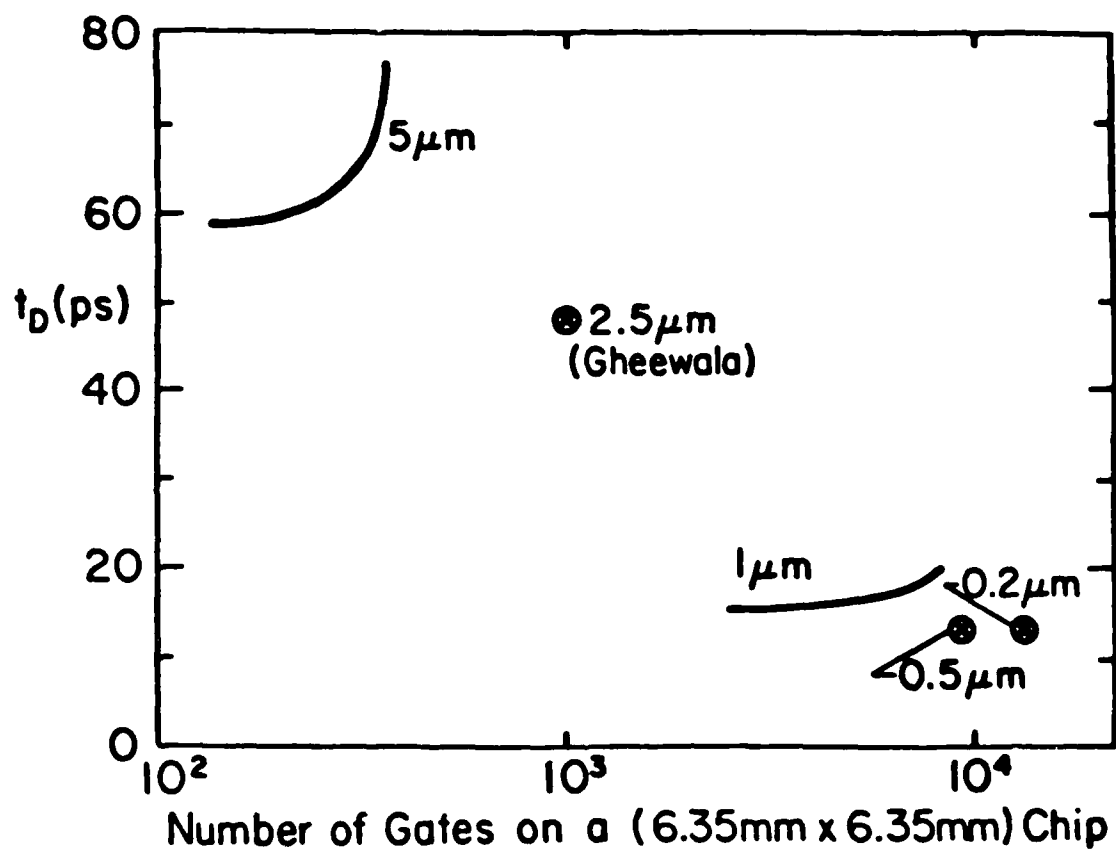


FIGURE 14
MINIATURIZATION CONSTRAINTS

a. $I_m R_L < V_g (= 2.5 mV)$
 $Z_0 = R_L$

(TRANSMISSION LINE CHARACTERISTIC
IMPEDANCE)

b. $I_m \gtrsim 100 \mu A$

(MINIMUM CRITICAL CURRENT
TO AVOID NOISE SWITCHING)

c. $J_c \lesssim 10^4 A/cm^2$

(MAXIMUM TUNNELING CURRENT DENSITY TO AVOID DETERIORATION OF I-V
CHARACTERISTIC)

d. $LI_m \approx \Phi_0$

(FOR INTERFEROMETER LOGIC)

(L = INTERFEROMETER LOOP INDUCTANCE Φ_0 = FLUX QUANTUM
CONDITION REQUIRED FOR SUITABLE INTERFEROMETER PROPERTIES)

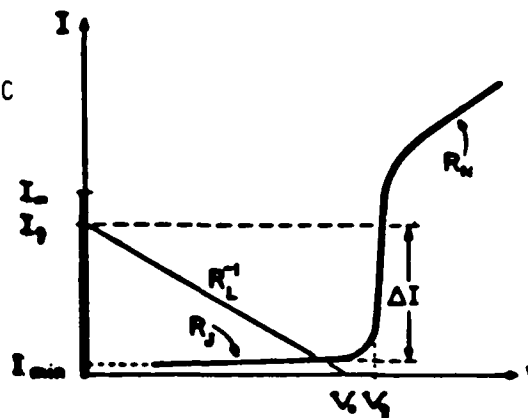
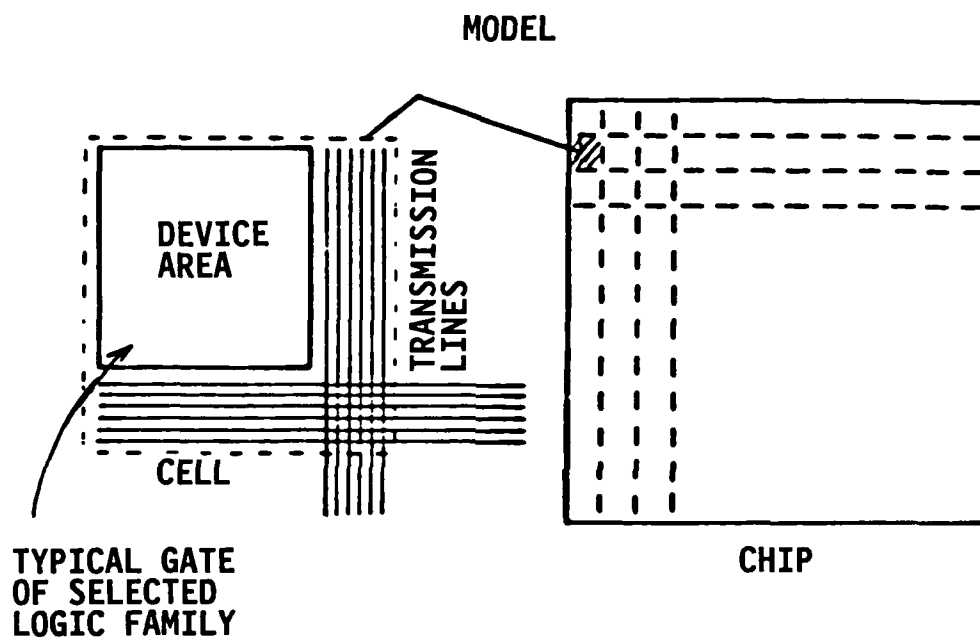


FIGURE 15
LOGIC CIRCUIT MINIATURIZATION STUDIES

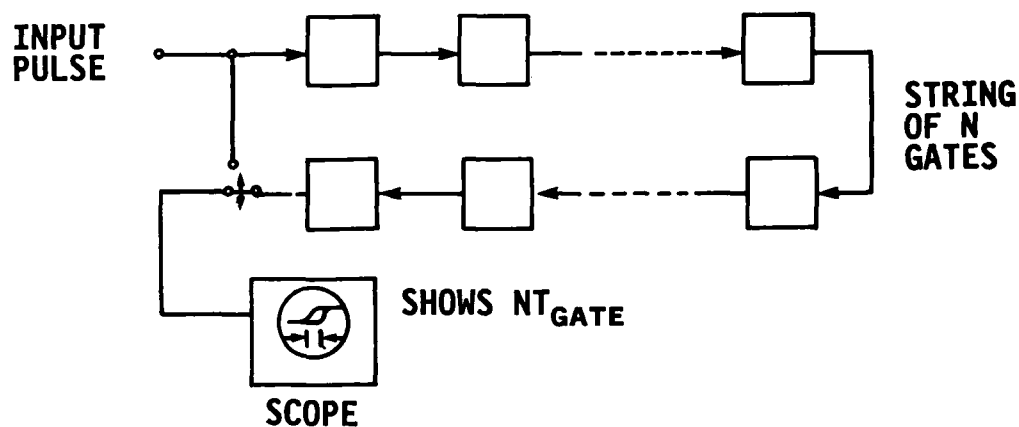


TWO LOGIC FAMILIES STUDIED

FIGURE 16

EXPERIMENT

(WILL ALSO MEASURE DELAY IN SINGLE GATE
WITH ON-CHIP SUPERCONDUCTIVE SAMPLER)



EVALUATE AT: $5 \mu\text{M}$
 $1 \mu\text{M}$
 $0.2-0.5 \mu\text{M}$

AND TEST SCALING CALCULATIONS

FIGURE 17
QUANTUM AND OPTICAL ELECTRONICS
EXAMPLES OF
JSEP-SUPPORTED WORK

S.E. SCHWARZ--PASSIVE MODE LOCKING OF CO₂ LASERS (C.V. SHANK, OBERT WOOD, T. DETEMPLE, A.V. NURMIKKO, P.L. GORDAN)

T.K. GUSTAFSON--OPTICAL INTERACTIONS IN TUNNELING BARRIERS (M. HEILBLUM, S.Y. WANG, M. GUEDES, C.W. SLAYMAN)

SHYH WANG--MODE CONTROL IN SEMICONDUCTOR LASERS (WON TSANG, D. BOTEZ, L. FIGUEROA, C.C. TSENG)

J.R. WHINNERY--THERMAL AND ACOUSTICAL INTERACTIONS WITH OPTICAL BEAMS (E.P. IPPEN, R.V. SCHMIDT, D. SOLIMINI, CHENMING HU, M.S. CHANG)

A. DIENES--DYE LASER MECHANISMS (ZAFER YOSA, RAVI JAIN, CHINLON LIN, O. TESCHICE, L. BRAVERMAN)

NOTE: MANY PROJECTS DONE JOINTLY

I would like to select one subject from our current work to describe in more detail. This is the subject of nonlinear coupling and control of supermodes in a semiconductor laser array. Semiconductor lasers are conveniently small, efficient, and easily modulated, but limited in power capabilities. The solution for higher powers has been to form arrays of individual lasers, fabricated on a single chip. The coupling among the individual laser elements produces higher-order modes, called "array modes" or "supermodes." The problem is controlling these to produce the desired radiation pattern.

Until recently the analysis of laser arrays has been analyzed by coupled mode theory--a passive theory. Much of the problem comes at higher pumping currents for which the active, nonlinear nature of the interactions must be considered. A self-consistent computer model was then developed, taking into account the interaction of fields and carriers, spontaneous emission, and diffusion of the carriers. Figure 18 shows a schematic of a six-element array using ridge-guiding for the individual elements. Spacings have been chosen on the basis of earlier simulations to favor the desired supermode. Figure 19 shows the result of calculations from the computer model for near-zone field patterns at threshold. The pattern for the fundamental mode shows nearly uniform strength for the six elements and is quite different from that calculated from passive mode theory. Figure 20 gives experimental results of power versus current, showing a very clean, linear curve, and Figure 21 shows the far-zone field, demonstrating single-lobe operation up to quite high currents.

FIGURE 18

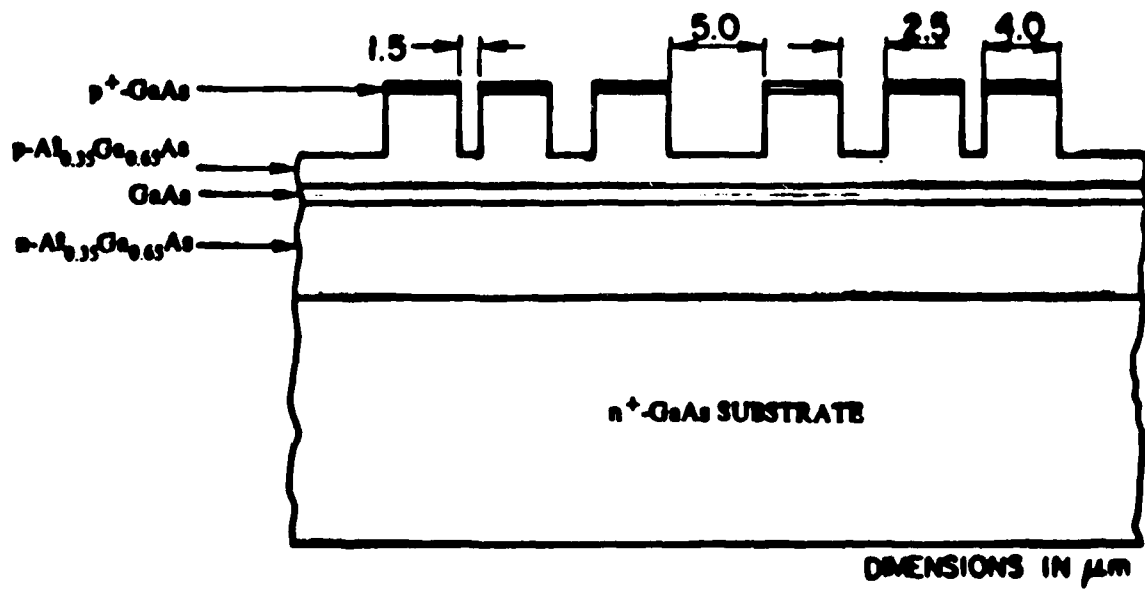
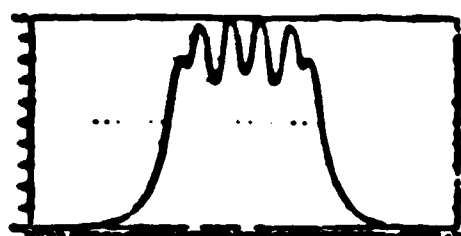
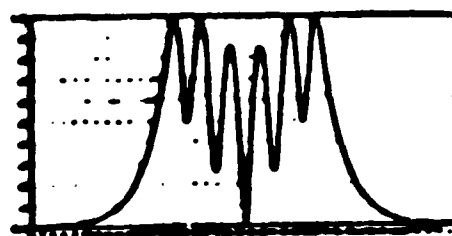


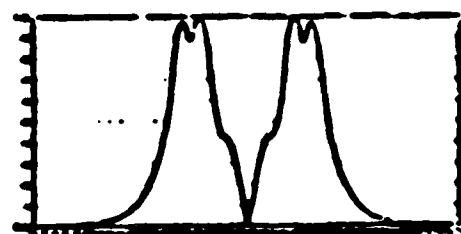
FIGURE 19



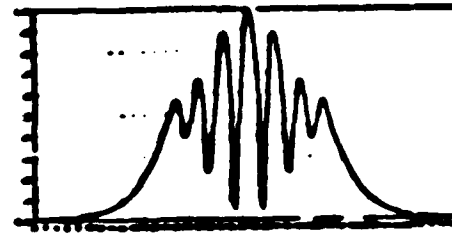
(a) $\nu=1$



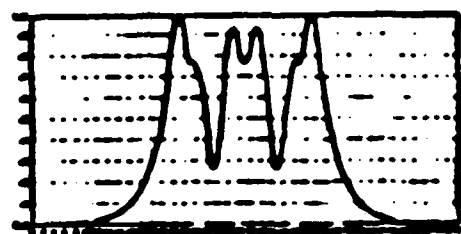
(e) $\nu=6$



(b) $\nu=2$



(f) $\nu=7$



(c) $\nu=3$



(g) $\nu=5$



(d) $\nu=4$

FIGURE 20

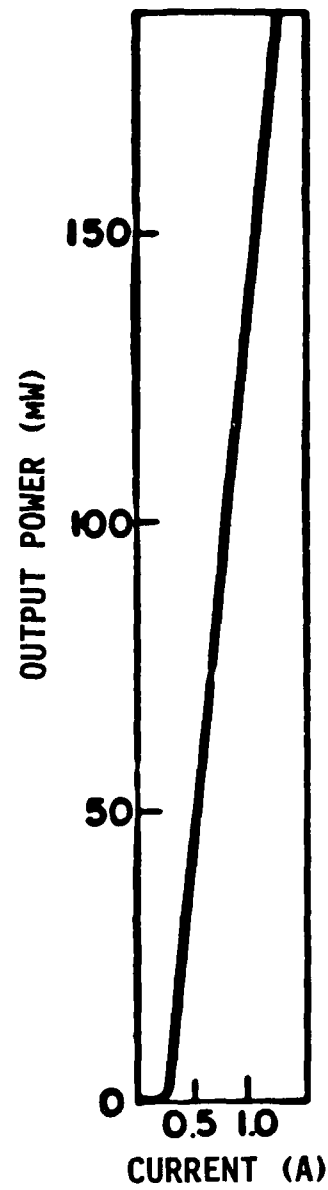
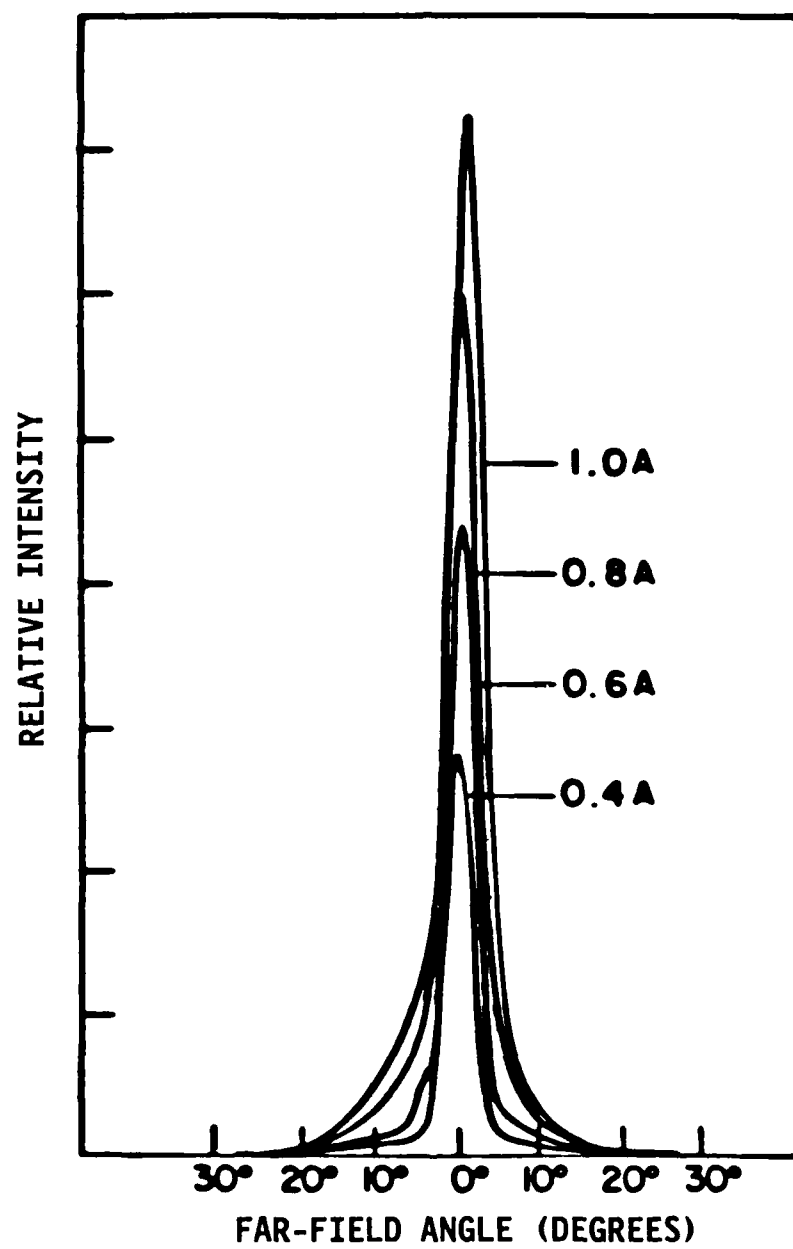


FIGURE 21



Gains to the various supermodes were also calculated from the model, showing the difficulty in keeping these separated, but also giving clues on how to do it. In a current JSEP-supported project, we are using gradient-index lenses as Fourier transformers to spatial filters in order to favor the desired pattern.

This project was a joint project with Prof. Wang, Dienes, and myself, with graduate students Yihjye Twu, Wei Hsin, and Constance Chang-Hasnain. It utilized interaction between theory and experiment, and built upon the microfabrication facilities and the computer-modeling technique we have seen in other projects.

These examples, selected from different fields, have shown some of the important interrelationships and the key role that JSEP played, especially in support of many projects at an early and critical stage. Thus, my view of the strength of JSEP is shown in Figure 22. The flexibility, especially in starting new and promising programs, is most important. The occasional equipment grants have been critical to us. JSEP has encouraged--more than most sponsoring agencies--interaction among the investigators, the review process, and interaction among the Services. It is a model for productive research support. Our congratulations and thanks to all who kept it so productive for these 40 years!

FIGURE 22
ADVANTAGES OF JSEP

- o **FLEXIBILITY**
- o **IMPORTANCE OF THE KEY EQUIPMENT GRANTS**
- o **INTERACTION AMONG INVESTIGATORS**
- o **INTERACTION AMONG THE SERVICES**

RESULTS

- o **IMPORTANT RESEARCH**
- o **PREPARATION OF FUTURE LEADERS IN THE FIELD**

*Joint Services Electronics Program
Research in Signal Processing*

Dr. Thomas Kailath
Professor, Associate Chairman
Electrical Engineering Department
Stanford University

I have a very particular reason for being grateful to and proud of the Joint Services Electronics Program since, but for it, I probably would have been in quite a different profession. In India, the chief ambition of most middle class parents was for their children to pass a national competitive exam and get a job for life in the government civil service. My parents were no different; they did not particularly mind what I studied in college, as long as I promised them that after I graduated I would take this exam and do whatever then befell me. I had no particular reason to think of doing anything else because studying abroad was not financially realistic for us. However, quite early in engineering school, I happened to read an article in *Popular Science* about information theory. That really caught my fancy. So without telling anyone I wrote to Harvard and MIT, and fortunately was accepted at both of them, with a teaching assistantship at Harvard and a research assistantship at MIT (which I discovered later was funded by JSEP).

A recollection of my first days at MIT was an interview with Prof. Henry Zimmerman, who spoke so engagingly earlier today. When I asked whether I could audit a particular course (beyond the 6 units permitted by the research assistantship, i.e., during my 34 "research" hours), Henry's response was, "There are 168 hours in a week. You can do whatever you want with your other 128 hours."

I also would like to relate a story about Arnie Shostak, then of ONR, to add to the several others we have heard already. He was responsible for a very unique experience that maybe some of you have shared: thanks to Arnie,

for a brief time I was a fairly senior officer in the U.S. Navy, while still a graduate student at MIT! In 1960, I was fortunate enough to have a paper accepted at an international conference. Since I was supported by JSEP, Ralph Sayers, of MIT's Research Laboratory of Electronics, arranged through ONR to get a military air transport ticket to Europe. I went to McGuire Air Force Base to catch the plane. The sergeant sitting at the desk had various questions for me, most of which I did not understand. One of these questions was, "What is your GS rating?" I did not know what that meant and just shrugged my shoulders. I forget what the sergeant said, but he wrote something down. When the plane landed in Newfoundland, we all got out to stretch our legs. There was a jeep waiting at the foot of the stairs and they called my name; they were looking for me, and saluted when I identified myself. It turns out they had scanned the passenger list and noted a senior officer on this plane and they were waiting to take me to the Officer's Club for a drink. I was too stunned to find out exactly what my rank was!

Both at MIT, and then coming to Stanford a few years later, I was supported by JSEP in many aspects of my research in communication theory and signal processing. I have worked in a number of different areas. Reflecting on those experiences, I can say that it almost always was JSEP support that was available for new investigations, because you did not have to explain to anyone, other than the local director, what you had in mind to do, or hoped to do. You can see that I have some very special reasons for saying thank you to all of you connected with the Joint Services Electronics Program.

I am not going to review all of the extensive work in electronics research in the JSEP program at Stanford. Rather, following Dr. Suttle's suggestion, I will talk somewhat more broadly at a "Scientific American" level, about some of the work going on in signal processing. I thought I would focus on three topics: 1) information theory and the foundations of that wonderful subject, 2) some work about 20 years ago on adaptive filtering by Prof. Bernard Widrow, at Stanford, and 3) some work under our current JSEP program that seems very promising.

Basically this talk is going to be about signal processing, particularly about statistical signal processing. This subject really got started after World War II. Furthermore, it may be said that most of the development of the statistical part of signal processing is due to JSEP. (Incidentally, for many people, statistics is sort of a red flag. One of those people was Winston Churchill. Recall his famous quotation was about the three kinds of lies: lies, damn lies, and statistics.) Statistics comes into the domain of the electronics research program, in the main, through trying to account for the effects of random noise. Until noise studies had been pursued and properly understood, many misleading papers in communications had been published, including one by a famous mathematician at the Bell Telephone Laboratories, who, in 1921, dismissed frequency modulation as completely impractical. Noting that the bandwidth of FM was very large, he made the famous remark that unfortunately, "noise, like the poor, will always be with us." He was wrong, as you know, as demonstrated by Armstrong, who showed that because of the fact that FM was of constant amplitude and one could understand its

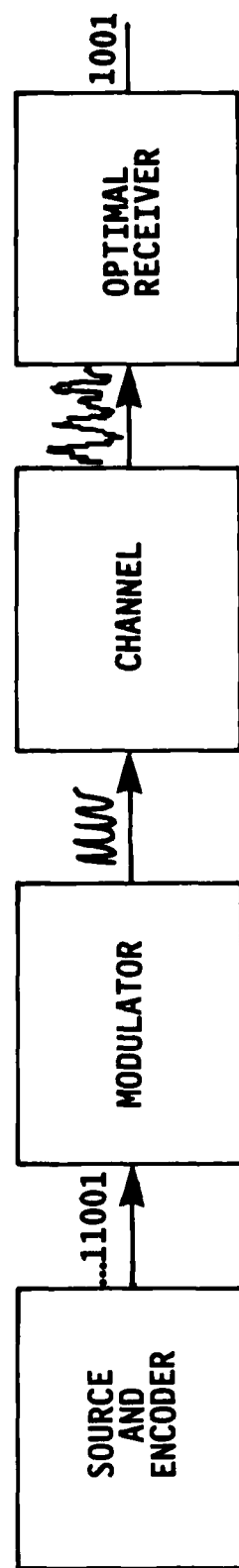
bandwidth properties, one could find a way of distinguishing signal from noise, the latter having amplitude fluctuations. So, after that episode, people were a little more humble when talking about noise, and this in turn stimulated many theoretical and practical investigations in that area of study.

The conventional wisdom at that time was that, by clever design, use of good amplifiers, and so on, the effects of noise could be made arbitrarily small. But, there was a price for this; namely, to get higher reliability you had to slow down the signaling rate (talk more slowly, or use repetition) so that in the limit, for perfect accuracy, you were saying the same thing over and over again; i.e., the signaling rate was zero. This seems to be reasonable, because thermodynamics and other fields have similar concepts: the most efficient engines deliver zero work, and so on. Therefore, it was a real conceptual surprise for people when Shannon, in 1947, said that that was not necessarily true! One could get communication with arbitrary accuracy and still maintain a nonzero signaling rate. Moreover, this rate could be fairly large, provided it was not larger than something called the capacity of the channel. Now the key idea behind Shannon's remarkable theorem is statistical. In particular, it is the law of large numbers, which says that statistical phenomena have a certain stability; that is, if one has a very large sample of noise, one can make very precise assertions about its behavior. Shannon's insight was that in the communications problem, one can then choose the information bearing signals cleverly so as to outwit the noise.

I will try to convey to you how that works out in a very simple case. First of all, let me give you a simple quantitative example to show you why people believed that you needed to reduce the signaling rate arbitrarily to get arbitrary accuracy. Then I will show you, in that same example, how the law of large numbers helps in understanding Shannon's remarkable way around this apparently inevitable conclusion. (Before going on, though, I must mention Prof. Norbert Wiener's important influence; many of us, including Shannon, owe a great debt to Wiener for emphasizing the statistical aspects of the communication problem.)

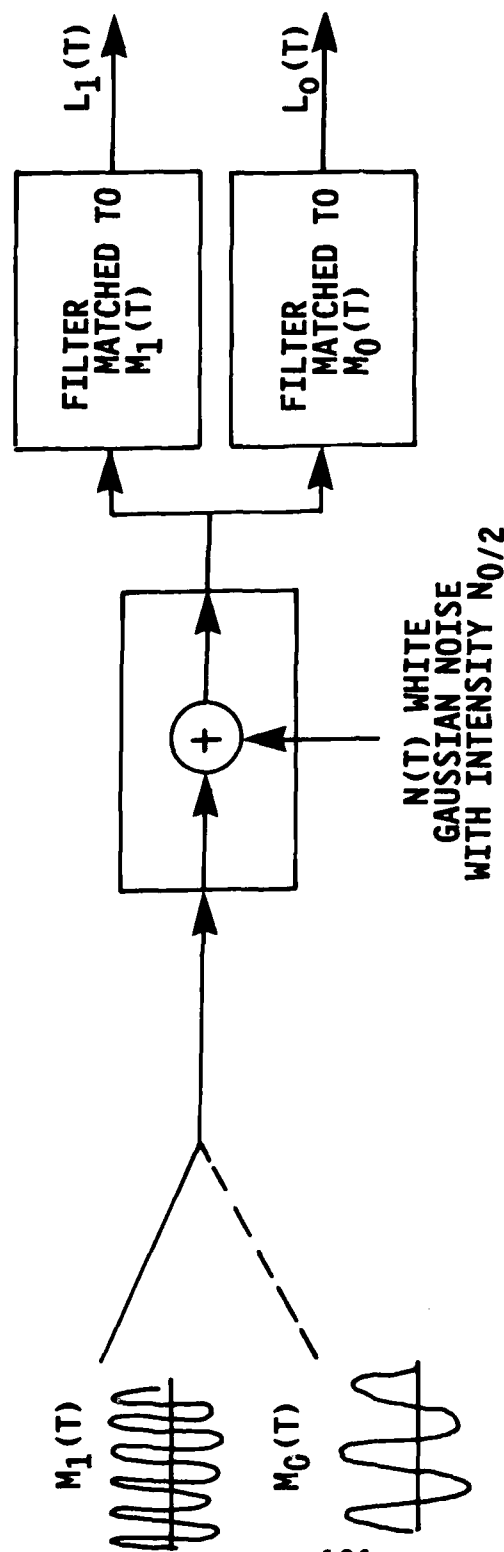
I shall take a very simple digital communication problem in which we want to send a string of binary digits through a channel to a receiver (Figure 1). To accomplish this, one reserves some time, say T seconds, for transmitting one or the other of two waveforms, one corresponding to the binary 1 and the other to zero. The signaling rate, using a logarithmic measure, is the logarithm of the number of messages per unit time, i.e., $\log_2 (2/T) = 1/T$ bits per second. See Figure 2 for a specific choice of communication channel, one that just adds noise to the transmitted waveforms (as in an idealized "free space" channel). The optimum receiver does a cross-correlation operation on the received data, the possible transmitted signal is multiplied by the received signal, and an average is taken, giving two numbers, $L_1(T)$ and $L_0(T)$, one for each possible signal. (The $\{L_1(T), L_0(T)\}$ also can be obtained by a passive operation called matched filtering). One then decides which message was sent, determined by whether or not $L_1(T)$ exceeds $L_0(T)$. If it does, decide a 1 was sent, if not, decide that a zero was sent. That is the scheme.

FIGURE 1
A SIMPLE DIGITAL COMMUNICATION SYSTEM



- o RESERVE A TIME T FOR EACH BINARY DIGIT
- o SEND A WAVEFORM $\left\{ \begin{matrix} M_1(T) \\ M_0(T) \end{matrix} \right\}$ FOR A $\left\{ \begin{matrix} 1 \\ 0 \end{matrix} \right\}$
- o SIGNALING RATE, $R = \log_2 M/T = \log_2 2/T = 1/T$ BITS/SEC
- o ERRORS ARE UNAVOIDABLE, BUT IT SEEMS REASONABLE THAT THE PROBABILITY OF RELIABLE COMMUNICATION CAN BE MADE ARBITRARILY CLOSE TO 1 IF THE SIGNALING RATE IS MADE SUFFICIENTLY SMALL.

FIGURE 2
FREE SPACE CHANNEL



$$P_{AV} = 1/T \int_0^T M_I^2(T) DT$$

$$L_I(T) = 1/T \int_0^T Y(T) M_I(T)$$

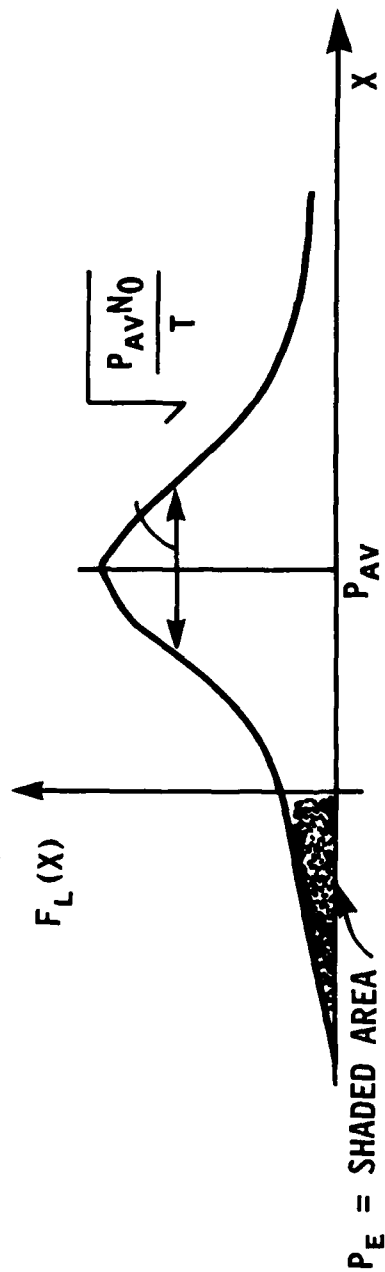
o DECIDE A 1 WAS SENT IF: $L = L_1(T) - L_0(T) \geq 0$

Because of the noise, there will be a probability of error in the decision process. It can be shown (see, e.g., Wozencraft and Jacobs, 1969) that, under a Gaussian assumption for the adaptive noise, the error will be as shown in Figure 3; it is the shaded area under a certain Gaussian (bell shaped) curve. You may now see that if one wants a lower error, the signal time duration should be increased (assuming a fixed average power P_{av}). When T is increased, the variance of the Gaussian distribution gets smaller, and therefore the area under the tail is reduced. Clearly, the probability of error may be made as small as desired by making T large enough. However, when T is extremely large, the signaling rate, $1/T$, gets smaller and smaller, and tends to zero as the desired error probability tends to zero. This behavior is the reason why people in the early days of communications theory seemed to believe that no matter what they did, the trade-off between error probability and signaling rate had its price.

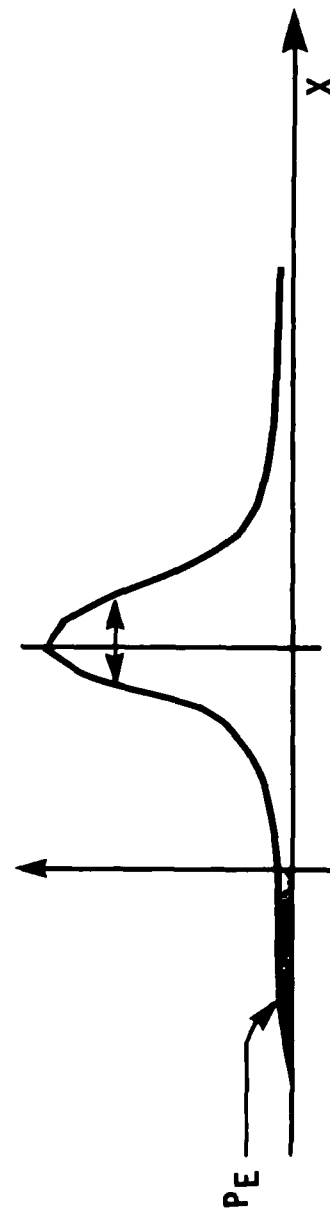
In his talk, Prof. Townes mentioned that many people were close to his idea for the maser. This is almost true in information theory, in the sense that some others might have thought of it, if their thinking had been bold enough to go beyond the conventional wisdom. For example, there was a Ph.D. thesis on the communication problem in the Soviet Union by a recent Vice-President of their Academy of Sciences, a radio engineer named Kotelnikov, who simultaneously with Dr. Siegert of the MIT Radiation Laboratory, suggested the use of multilevel signaling, so-called M-ary signaling. Instead of sending one waveform for a 1, and another for a zero, these people suggested breaking up the data into blocks, say of length 3. Then, in each block

FIGURE 3
PROBABILITY OF ERROR

- o SUPPOSE $M_1(T)$ WAS SENT. THERE WILL BE AN ERROR IF $L < 0$.



- o CLEARLY, AS T APPROACHES INFINITE, THE SHADED AREA WILL DECREASE AND CAN BE MADE AS SMALL AS WE WISH BY TAKING T LARGE ENOUGH. HOWEVER, THE SIGNALING RATE, $R = 1/T$, WILL BECOME SMALLER AND SMALLER.....



there are 8 possible distinct sequences of length 3 (see Figure 4). By reserving a particular waveform for each of the 8 sequences, then making decisions on which of the 8 waveforms was sent, one may determine which of the 8 three-digit sets were sent. It turns out that the operations involved are still the same as in the two-waveform case, i.e., one performs cross-correlation (matched filtering) against each of the possible transmitted waveforms, then takes the largest output and decides what block of digits was sent. The signaling rate has gone up because there are now 8 possible messages in the time T , and, therefore the signaling rate is $3/T$ bits per second. In fact, one can get as large a rate as desired by using longer and longer such sequences in this technique.

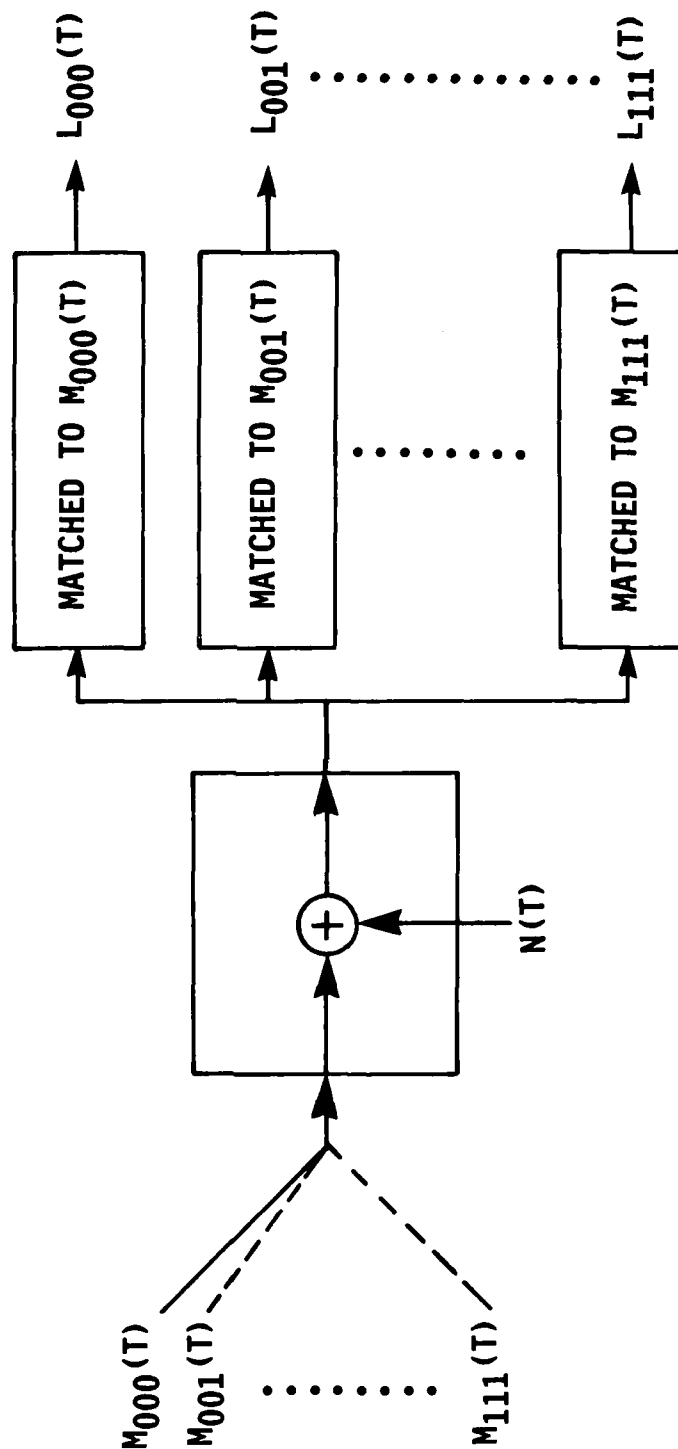
But how about the probability of error for such so-called M-ary signaling? Well, again because of the Gaussian distribution of the noise, one can plot the distributions of the outputs for each of the different cross-correlation (matched) filters (see Figure 5). We will assume that it was the waveform corresponding to the first sequence, 000, that actually was sent. Therefore, in the corresponding cross-correlation calculation, this output will have a Gaussian distribution with a nonzero mean and a certain variance. But the cross-correlator outputs corresponding to the other possible transmitted sequences all will have Gaussian distributions centered at zero mean, although of the same variance. Now if one makes T very large, the output distributions concentrate about their means, with the distribution corresponding to the first (000) sequence being (almost) a delta function at P_{av} and the distributions for the outputs of the other ("incorrect sequence") filters

FIGURE 4

KOTELNIKOV (USSR), SIEGERT (MIT RAD. LAB.) SUGGESTED THE USE OF M-ARY SIGNALING: BREAK UP THE DATA STREAM INTO BLOCKS, SAY OF LENGTH 3,

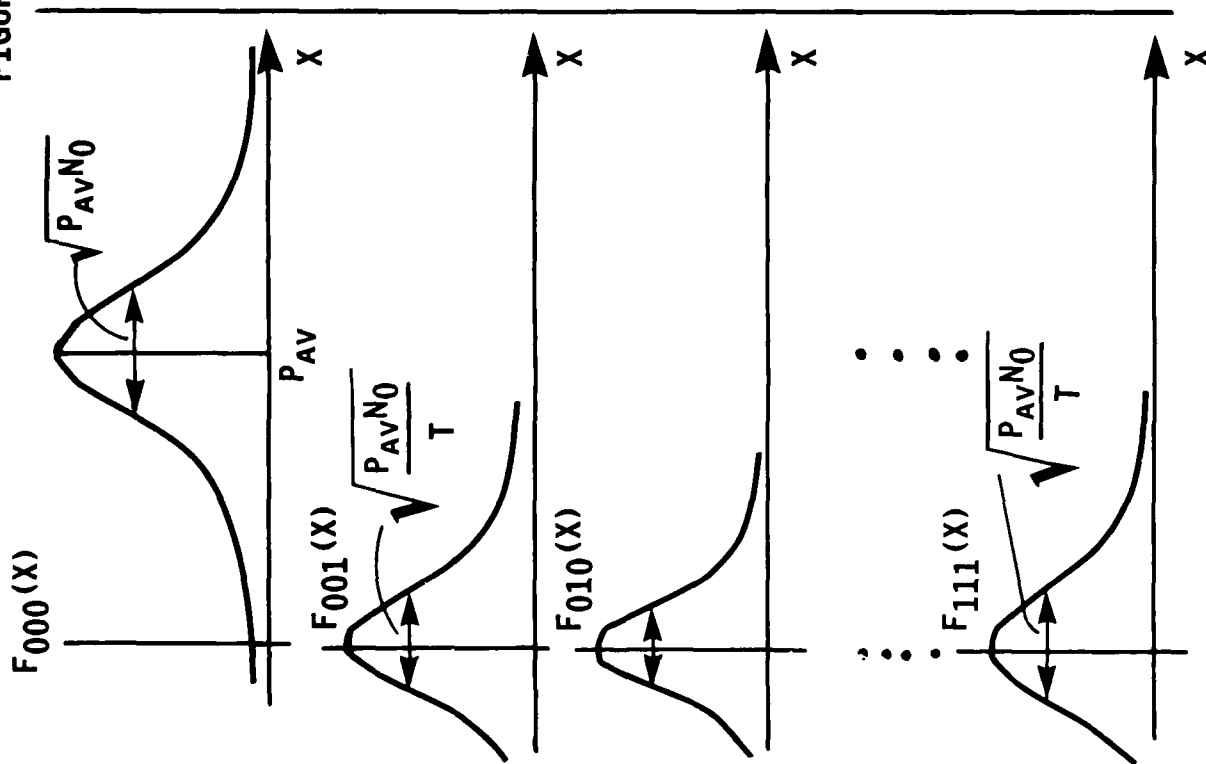
110 101 001
 $\underbrace{\hspace{1.5cm}}_T$

AND IN TIME T SEND ONE OF THE EIGHT POSSIBLE STRINGS OF LENGTH 3.



$$\text{SIGNALING RATE } R = \log_2 M/T = \log_2 8/T = 3/T \text{ BITS/SECOND}$$

FIGURE 5



- SUPPOSE $M_{000}(T)$ WAS SENT
- o THERE WILL BE AN ERROR IF $L_{000}(T)$ IS EXCEEDED BY ANY OTHER $\{L_{001}(T), L_{010}(T) \dots L_{111}(T)\}$
 - o THE PICTURES WOULD SEEM TO INDICATE THAT BY INCREASING T , $L_{000}(T)$ CAN BE MADE TO CONCENTRATE AT P_{AV} AND THE OTHERS AT 0, SO THAT THE PROBABILITY OF ERROR WILL BE VERY SMALL.
 - o BUT THIS WOULD INDICATE THAT BY BLOCKING INTO LARGER AND LARGER BLOCKS, WE COULD GET ARBITRARILY HIGH RATES $R = \log_2 M/T$ WITH VANISHINGLY SMALL ERROR PROBABILITY!?

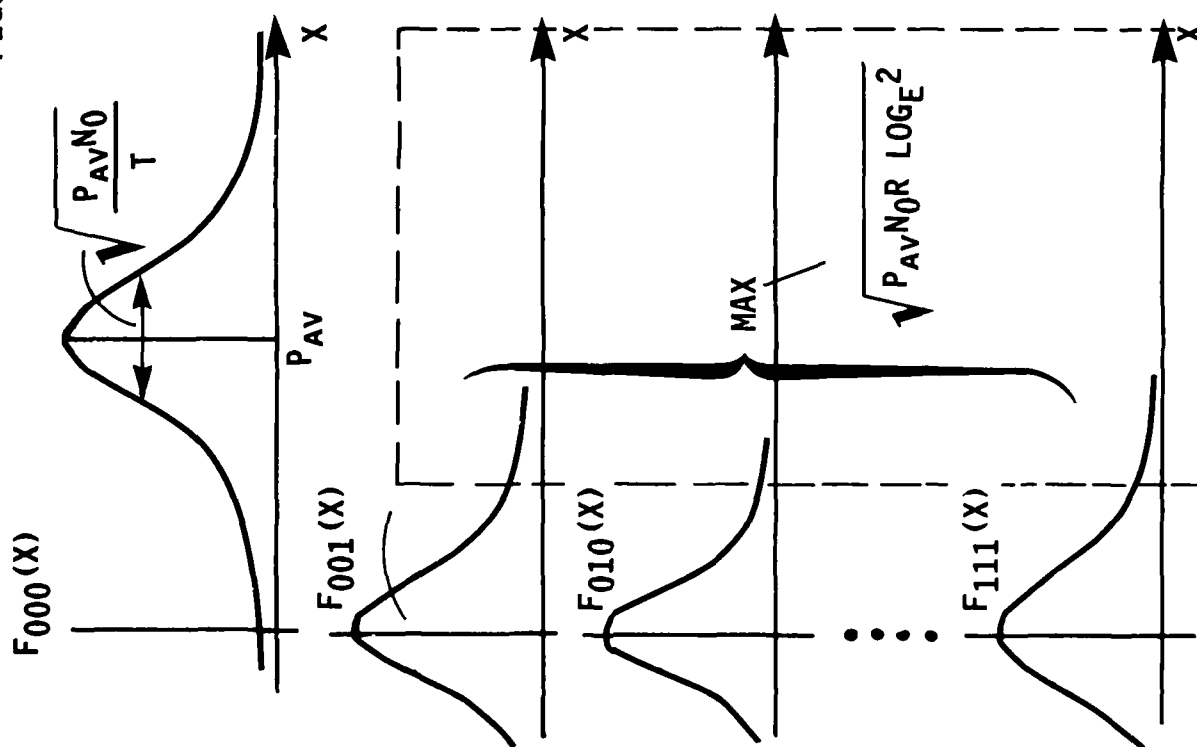
being (almost) delta functions at zero. (See Figure 5). It would appear that one would be able to distinguish the correct signal, which will almost certainly be equal to P_{av} , from the other outputs, which will almost certainly be zero. Therefore, the probability of error would appear to be very small. Of course, the signaling rate, $3/T$, would still tend to zero with T . But if, as we went to larger T , we also increased the number, M , of possible transmitted signals, i.e., if we broke up the data stream into longer and longer blocks, we could keep the signaling rate constant. More precisely, if we choose M as $M = 2^{\alpha T}$, where α equals some fixed number, then the signaling rate, $R = \log_2 M/T$, would be equal to α . It seems that we could choose any α , and a suitably large T , and by the above argument apparently always guarantee that the cross-correlator output for the correct sequence always would be larger than the (zero) value of the cross-correlator outputs corresponding to the other possible transmitted messages. All of a sudden the seeming impasse has been broken! And how: we seem to have arbitrarily low error at any signaling rate!

But here is where the statistics come into play. We already have used it partly because we have said that things (the distributions of the correlator outputs) converge and settle around their mean; but we have overlooked something. Even though each of the "incorrect" random variables is individually small, if there are many of them, there is a reasonable probability that at least one of those very small ones may be large. That is not very surprising if you stop to think about it. In fact, it turns out that the following is true: even though individually each of a collection of random variables may have

(with a probability of almost 1) a nonzero value, which depends on the average power and noise, and the number of messages. That is how the number of messages, M , comes into the picture. If we choose a value for M that is too large, the largest of the incorrect outputs still will assume some nonzero value, which will grow with M . But as long as we know how things will behave (by the statistical laws of large numbers), we still can arrange things to our advantage. Thus, Figure 6 shows what one should do and also shows the value assumed in the limit by the largest of M Gaussian random variables. If one wants perfect reliability, one should make sure that P_{av} , the value that the correlator output corresponding to the correct (i.e., transmitted) sequence will take, will be larger than the largest of the "incorrect" values. Then with probability 1, one never makes an error. But as shown in Figure 6, this choice implies signaling at a certain rate, which is nonzero, although constrained by the upper bound in this inequality. This was Shannon's revolutionary insight, illuminated in retrospect through this simple example. Kotelnikov and Siegert could no doubt have obtained the same result, if only they had had the genius to look for that seemingly inconceivable possibility of arbitrary reliability at a nonzero signaling rate.

In summary, what Shannon realized (see the quotation shown in Figure 7) is that we should try to arrange to use the laws of statistics to tell us what the noise is doing, and then try to be clever about choosing the signals and the rate. That is the central idea of information theory. There are many other remarkable aspects of the theory, e.g., the proof that the above procedure always will work.

FIGURE 6



- o NO!! EVEN THOUGH THE "OTHER" $L_{***}(T)$ ARE INDIVIDUALLY SMALL, IF THERE ARE MANY OF THEM THERE IS A NONZERO PROBABILITY THAT ONE OF THEM MIGHT BE QUITE LARGE. IN FACT IT TURNS OUT THAT FOR LARGE M

$$\text{MAX} \{L_{***}(T)\} \rightarrow \sqrt{\frac{P_{AV} N_0 \text{LOG}_2 M \text{LOG}_E 2}{T}} = \sqrt{P_{AV} N_0 R \text{LOG}_E 2}$$

- o WE NOW HAVE THE SOLUTION: FOR LARGE T, AND CONSEQUENTLY LARGE M $= 2^{RT}$

$$L_{000}(T) \rightarrow P_{AV}; \text{MAX}\{L_{***}(T)\} \rightarrow \sqrt{P_{AV} N_0 \text{LOG}_E 2}$$

- o WE CAN GUARANTEE NO ERROR BY HAVING

$$\sqrt{P_{AV} N_0 R \text{LOG}_E 2} < P_{AV} \text{ OR } R < \frac{0.72 P_{AV}}{N_0} \text{ BITS/SEC}$$

- o THAT IS, WE CAN HAVE ARBITRARILY RELIABLE COMMUNICATION AT A NONZERO SIGNALING RATE R, PROVIDED R IS NOT TOO LARGE (LESS THAN THE CHANNEL CAPACITY)!

FIGURE 7

"DELAY HAS THE (ADDITIONAL) FUNCTION OF ALLOWING A LARGE SAMPLE OF NOISE TO AFFECT THE SIGNAL BEFORE ANY JUDGEMENT IS MADE AT THE RECEIVING POINT AS TO THE ORIGINAL MESSAGE. INCREASING THE SAMPLE SIZE ALWAYS SHARPENS THE POSSIBLE STATISTICAL ASSERTIONS."

- SHANNON, A MATHEMATICAL
THEORY OF COMMUNICATIONS, SEC. 19

It has taken awhile, but applications of Shannon's ideas now are starting to blossom. Some of the more widely used applications are in error-correcting codes, which now are used in the technology of compact audio discs, where the so-called Reed-Solomon codes are used. Sending digital data over the voice channel at rates two or three times as fast as anybody had thought feasible is a recent application of information theory; others arise in cryptography and in the field of data compression (Figures 8, 9 & 10.)

I am happy to say that the Joint Services Electronics Program has been by far the major sponsor of information theory research in the country.

Let me now switch to another application of statistical communications theory, also heavily fostered by JSEP. This application is related to Prof. Norbert Wiener's ideas of adaptive filtering.

Consider a signal corrupted by some sort of noise, which one wishes to remove. One way of attaining this goal is to use a feedback scheme in which the error signal (see Figure 11) is used to control the parameters in an adaptive scheme for cancelling an interfering signal (noise). The above technique has found application in many engineering systems. A U.S. Navy application (started under JSEP sponsorship of Prof. Bernard Widrow's work at Stanford) involved suppression of interfering signals in antenna arrays used for direction-finding. By using a variation of the adaptive feed-back loop shown in Figure 11, Prof. Widrow and his students were able to make the radiation pattern of the antenna array maximize its beam in the

FIGURE 8
APPLICATIONS

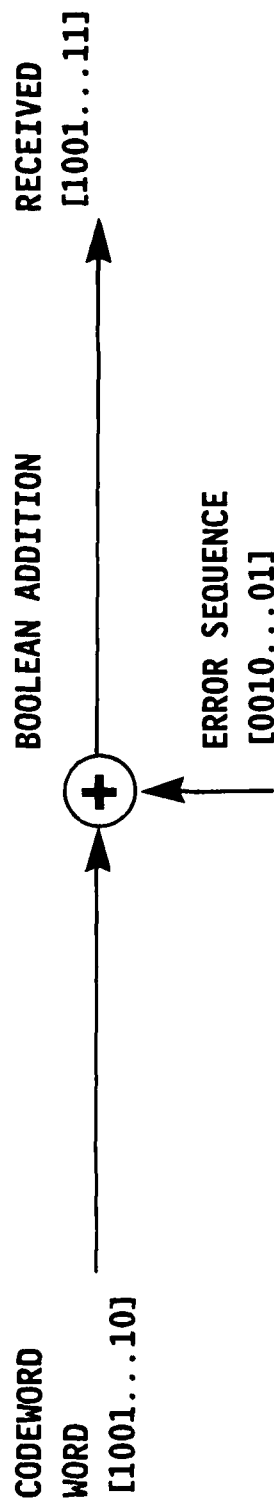
- o **ERROR CORRECTING CODES:** REED-SOLOMON CODES
BOSE-CHAUDHURI-HOCQUENGHEM
(BCH) CODES
CONVOLUTIONAL CODES

SUCCESSFULLY USED IN DEEP-SPACE COMMUNICATIONS AND IN
COMPANY DISC SYSTEMS
- o **TRELLIS MODULATED WAVEFORMS HAVE BEEN DEVISED TO SEND
DIGITAL DATA AT RATES UP TO 19.8 KILOBITS/SEC OVER
ORDINARY 3KHz TELEPHONE VOICE CHANNELS. THIS IS MORE
THAN TWICE WHAT HAS PREVIOUSLY BEEN CONSIDERED
POSSIBLE.**
- o **ENCRYPTION SYSTEMS - PUBLIC KEY CIPHERS
- RSA ALGORITHM**
- o **DATA COMPRESSION SCHEMES
- ZIV-LEMPER
- VECTOR QUANTIZATION
-**

FIGURE 9

REED-SOLOMON CODES

o BINARY SYMMETRIC CHANNEL



- o T-ERROR CORRECTING REED-SOLOMON CODES ARE DEFINED BY THE FACT THAT THE DISCRETE FOURIER TRANSFORMS (SUTIABLY DEFINED, OVER AN EXTENSION OF THE GALOIS FIELD $GF(2^t)$) OF ALL THE CODEWORDS HAVE qt CONSECUTIVE ZEROS IN THE SAME LOCATIONS
- o THEREFORE ANY ONES IN THE (TRANSFORM OF THE) RECEIVED WORD IN THESE LOCATIONS ARISES FROM AN ERROR AND GIVES A "SYNDROME" THEREOF
- o THE KEY EQUATION FOR DECODING IS A SO-CALLED DIOPHANTINE EQUATION

FIGURE 10

THE KEY EQUATION

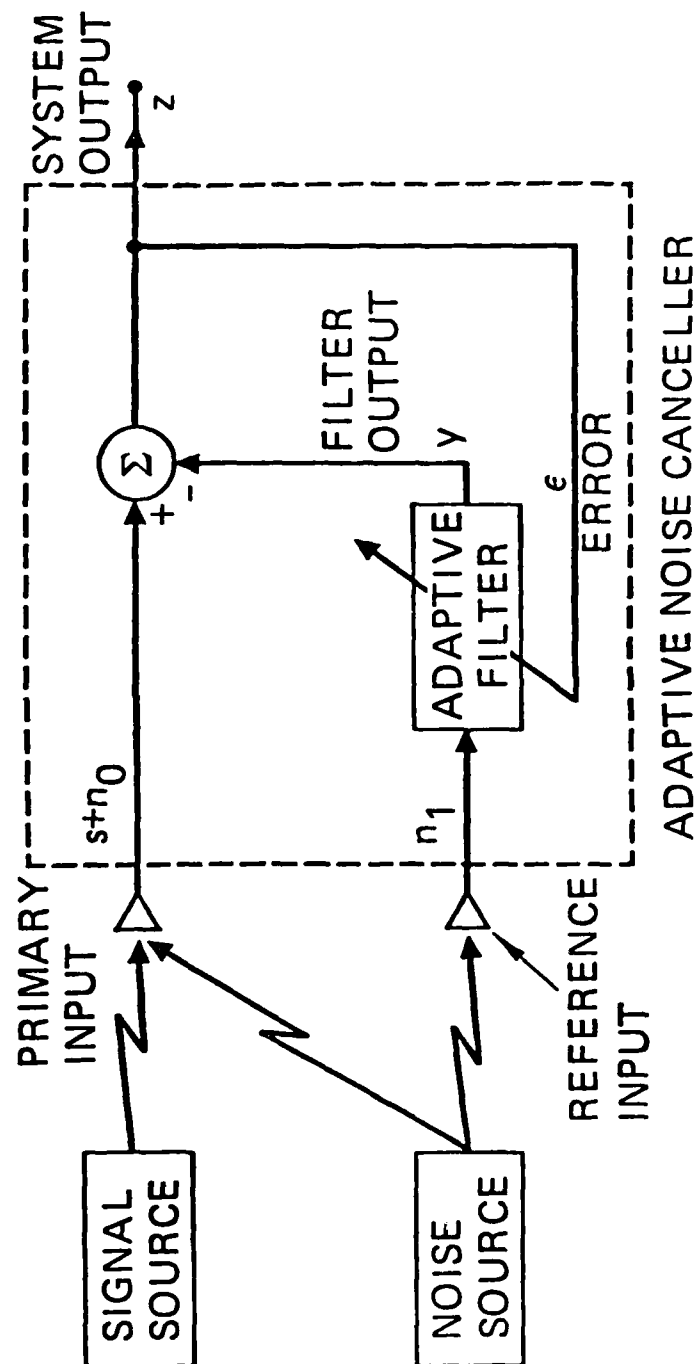
$$S(Z) = \frac{W(Z)}{\Lambda(Z)} \text{ MOD } Z^{2T}$$

SYNDROME POLYNOMIAL

ERROR LOCATION POLYNOMIAL

- o IT IS STRIKING THAT PERHAPS THE BEST WAY OF SOLVING THIS EQUATION IS BY A VARIATION OF ONE OF THE OLDEST ALGORITHMS ON RECORD, THE EUCLIDEAN ALGORITHM FOR FINDING THE GCD OF TWO INTEGERS
- o IMPLEMENTATION STRUCTURES WELL MATCHED TO PARALLEL PROCESSING AND TO V.L.S.I. CAN BE FOUND BY BRINGING IN IDEAS FROM TRANSMISSION LINE (SCATTERING) THEORY.

FIGURE 11
ADAPTIVE FILTERING



SUCH ADAPTIVE TECHNIQUES HAVE BEEN COMBINED WITH INFORMATION-THEORETIC IDEAS TO DEVISE SCHEMES FOR SENDING DIGITAL DATA AT RATES UP TO 19.8 KBITS/SEC OVER 3KHZ VOICE CHANNELS, MORE THAN THREE TIMES THE RATE PREVIOUSLY CONSIDERED POSSIBLE.

desired look direction, while developing a null in the (unknown) directions of the unknown interfering signals (see Figure 12).

These ideas are being used in numerous applications, military as well as civilian. (Monitoring the heartbeat of a fetus can be done in this way, by cancelling out the strong signal from the mother's heartbeat. Other examples may be found in echo suppression techniques used in long-distance telephone lines or satellite communications).

Some recent work in the area has involved the solution of a problem that has been outstanding for 20 years: how to deal with "coherent" interference. Two signals are coherent if one is a scaled and delayed version of the other. This happens in multipath and in smart jamming environments; in the latter, one picks up the signal and reflects it in a retrodirected mode. What happens is that if there are interfering signals that are completely correlated (coherent) with the signal, the normal schemes do not give nulls in the right directions, because they tend to get confused by the strong dependence between what is wanted and what one is trying to reject. We have been able to devise schemes that use some combination of physical ideas and mathematics to develop new arrays that put nulls in the proper direction (see Figures 13-15) even in this different problem of coherent interference.

Finally, I would like to describe some very recent work on the direction-of-arrival (DOA) problem. Let us say we are interested in knowing from which direction different signals are coming in to our antenna array (see Figure 16).

FIGURE 12

EVOLUTION OF THE DIRECTIVITY PATTERN WHILE LEARNING TO
ELIMINATE FIVE DIRECTIONAL NOISES AND UNCORRELATED NOISES

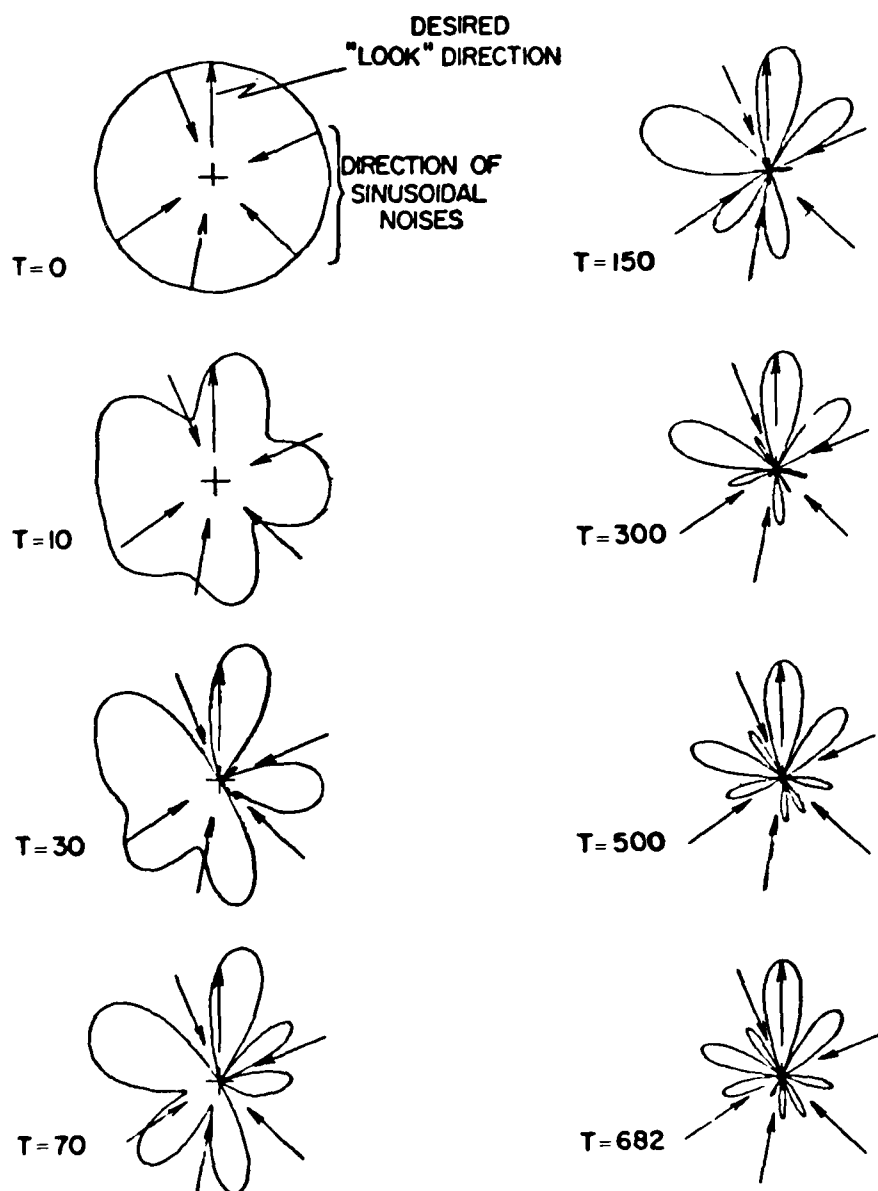


FIGURE 13
THE PROBLEM

CURRENT ADAPTIVE ANTENNA SYSTEMS TOTALLY FAIL IN COHERENT RECEIVING ENVIRONMENTS. (IF ONE SIGNAL IS A SCALED AND DELAYED REPLICA OF THE OTHERS, THEN SIGNALS ARE COHERENT)

EXAMPLES:

- 1. MULTIPATH PROPAGATION**
- 2. A "SMART JAMMER" THAT RECEIVES THE RADAR SIGNAL AND RETRODIRECTS THE SIGNAL TO THE RADAR RECEIVER.**

NEW TECHNIQUES FOR COMBATTING COHERENT INTERFERENCE

- T.J. SHAW & T. KAILATH
ASSP, JUNE 1985

FIGURE 14A
FROST ARRAY

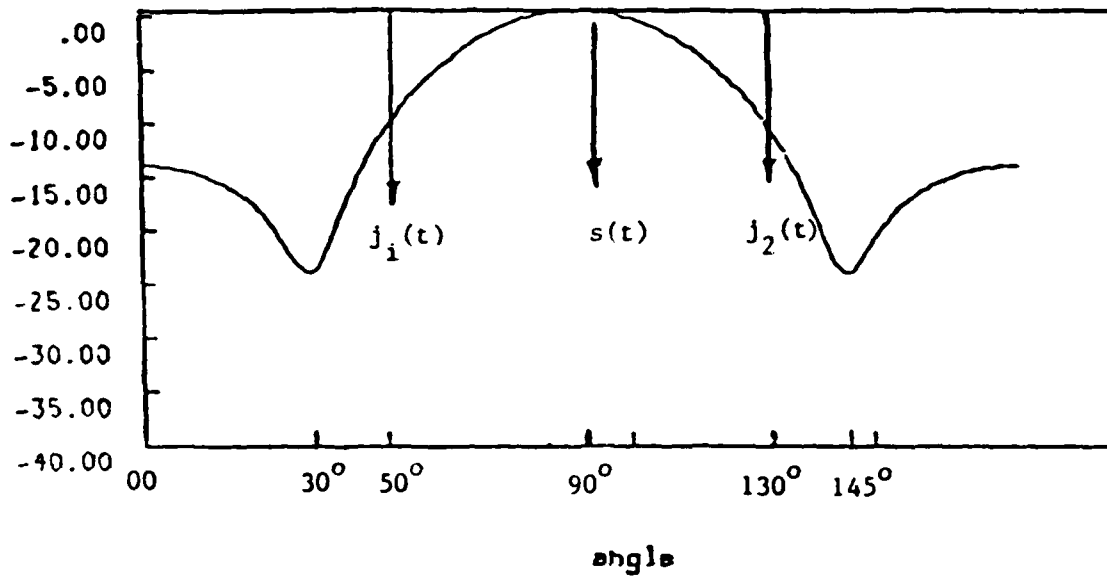


FIGURE 14B
NEW ARRAY

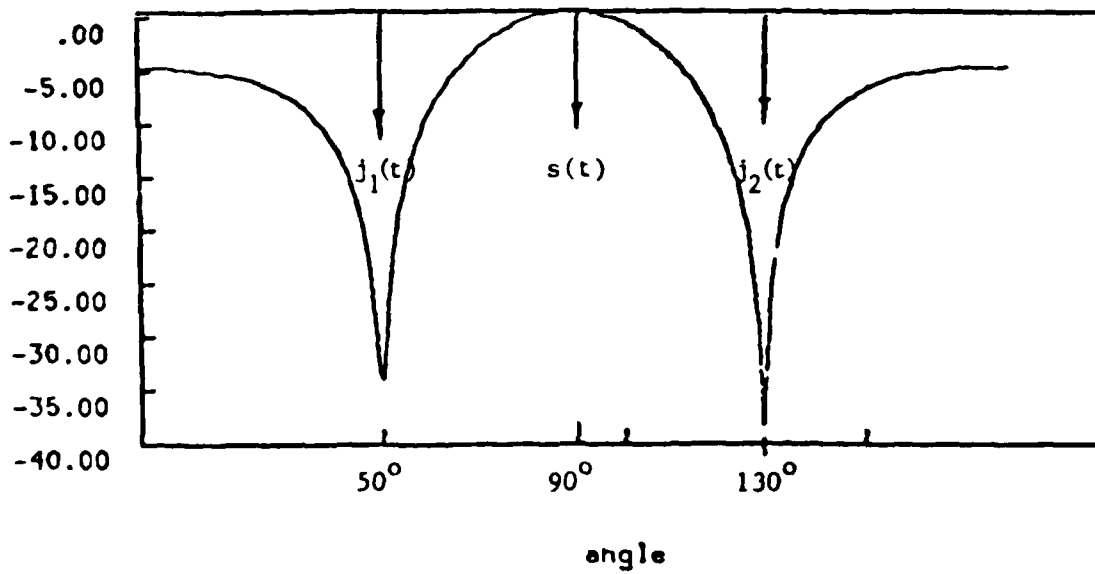


FIGURE 15A
INPUT SIGNAL

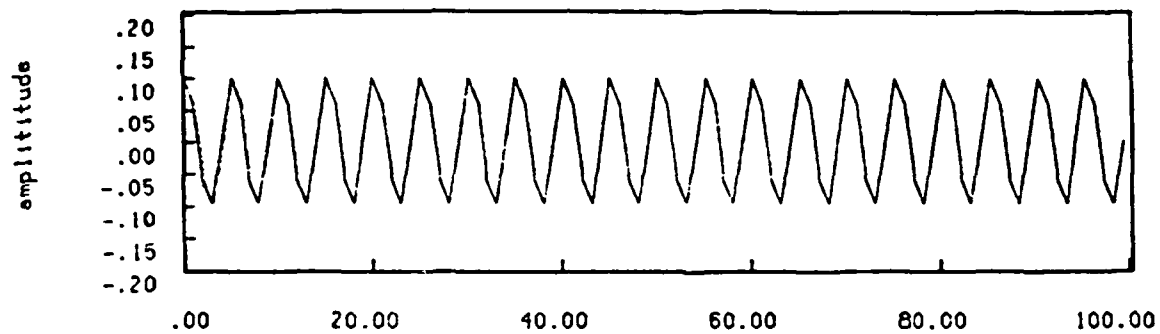


FIGURE 15B
OUTPUT SIGNAL OF FROST ARRAY

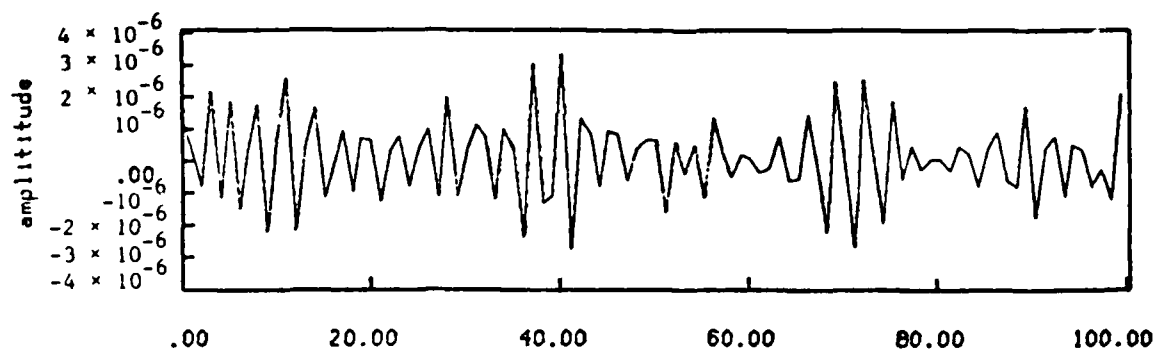


FIGURE 15C
OUTPUT SIGNAL OF THE NEW ARRAY

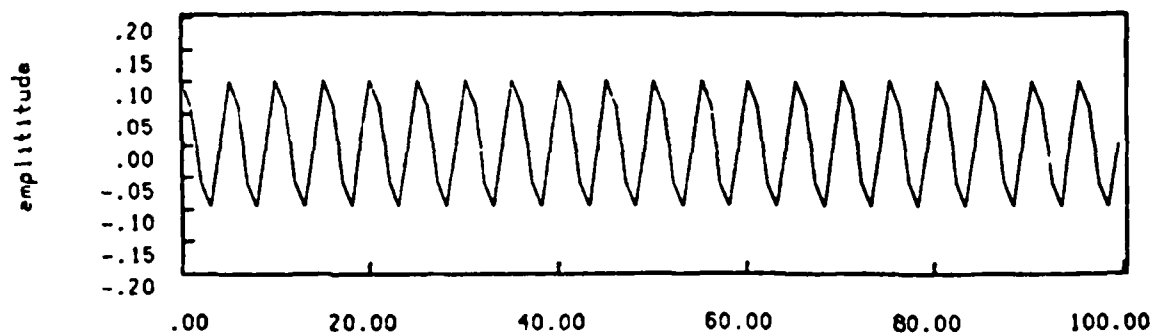
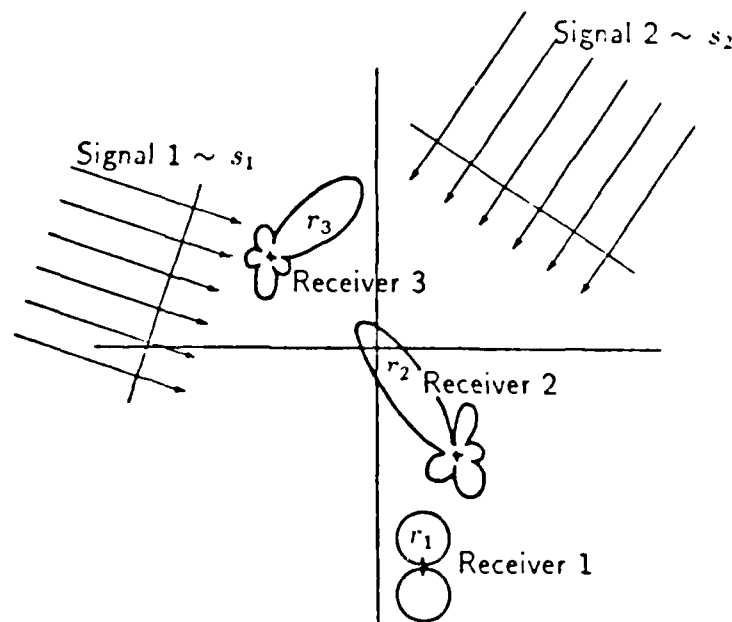


FIGURE 16
MULTIPLE SOURCE DIRECTION-OF-ARRIVAL ESTIMATION



PROBLEM--USING THE RECEIVED SIGNALS, KNOWLEDGE OF THE NOISE COVARIANCE, AND COMPLETE KNOWLEDGE OF THE ARRAY GEOMETRY AND SENSOR ELEMENT CHARACTERISTICS, ESTIMATE THE DIRECTIONS-OF-ARRIVAL (DOAs). THE SIGNAL COVARIANCE (POWERS AND CORRELATIONS), AND RECONSTRUCT THE SIGNALS INDIVIDUALLY (SIGNAL COPY).

D = NUMBER OF SOURCES

M = NUMBER OF SENSORS

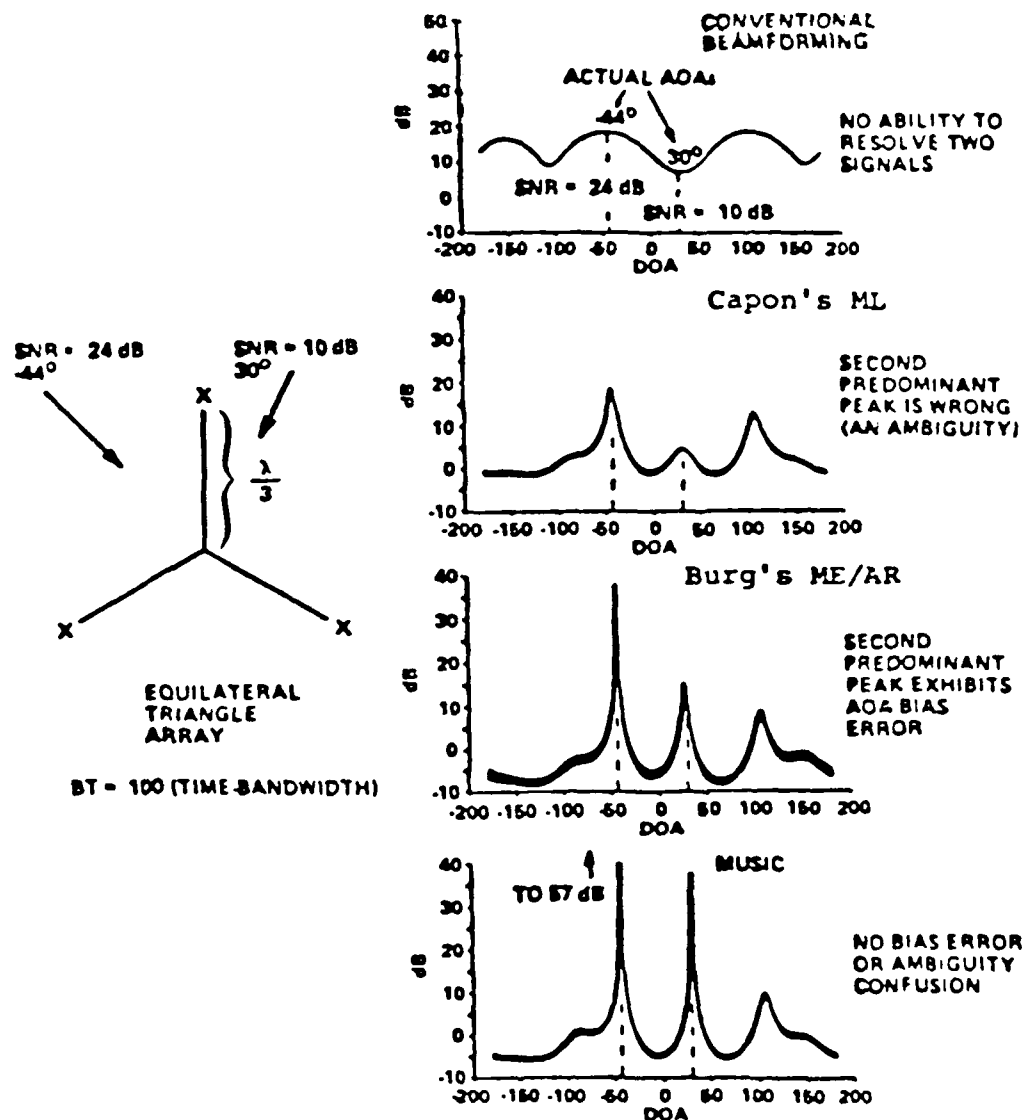
**$A(0) = [A_1(0), A_2(0), \dots, A_M(0)]^T$,
= ARRAY RESPONSE (STEERING, DIRECTION) VECTOR.**

**$X(T) = A(0_1)S_1(T) + A(0_2)S_2(T) + \dots + A(0_D)S_D(T) + N(T)$,
= $As(L) + N(T)$.**

Some of the conventional solutions are shown in Figure 17. They all have some problem or other (what doesn't?). Conventional beam forming generates many ambiguities because the beam patterns have low resolution. (Many signals may come into the shallow regions of the antenna pattern.) There is a so-called "maximum likelihood" method that was developed at Lincoln Laboratories in connection with seismic research some years ago. It is still, perhaps, the most widely used in the field. But the method is computationally expensive and has certain problems, especially ambiguous peaks.

A few years ago Ralph Schmidt, then at ESL, Inc. and later at Stanford, proposed an improved method called MUSIC. This scheme has very good properties, with very small ambiguities and errors. However, MUSIC methods also are handicapped in that they require a computationally intensive search. If the look angle of search is, say two radians, but one wants an accuracy of one milliradian, a very large number of calculations must be made. Another handicap is that one needs, in all of these schemes, full knowledge of the array geometry (which must be stored for use in the computation). Again through a combination of some physical reasoning and some mathematics, we have been able very recently (see Figure 18) to come up with a scheme that, although it still has certain restrictions (see Figure 19), can potentially be very useful. One restriction is that to use the technique successfully, one must be in the far field of the incident wave. The other is that the antenna system must consist of doublets, in each of which the gain pattern of each element in each doublet is the same. Moreover, all the doublets of these must be oriented

FIGURE 17
CURRENT TECHNIQUES FOR DOA ESTIMATION



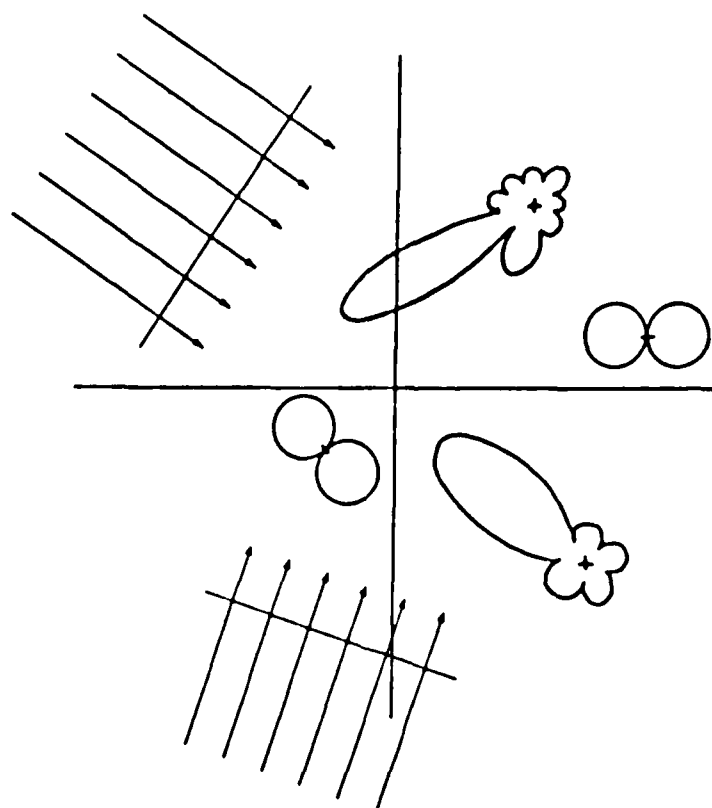
ALL THESE METHODS

- o REQUIRE A COMPUTATIONALLY EXPENSIVE SEARCH ON PARAMETER SPACE
- o REQUIRE FULL KNOWLEDGE OF THE ARRAY

FIGURE 18
ESTIMATION OF SIGNAL PARAMETERS VIA ROTATIONAL
INVARIANCE TECHNIQUES - ESPRIT

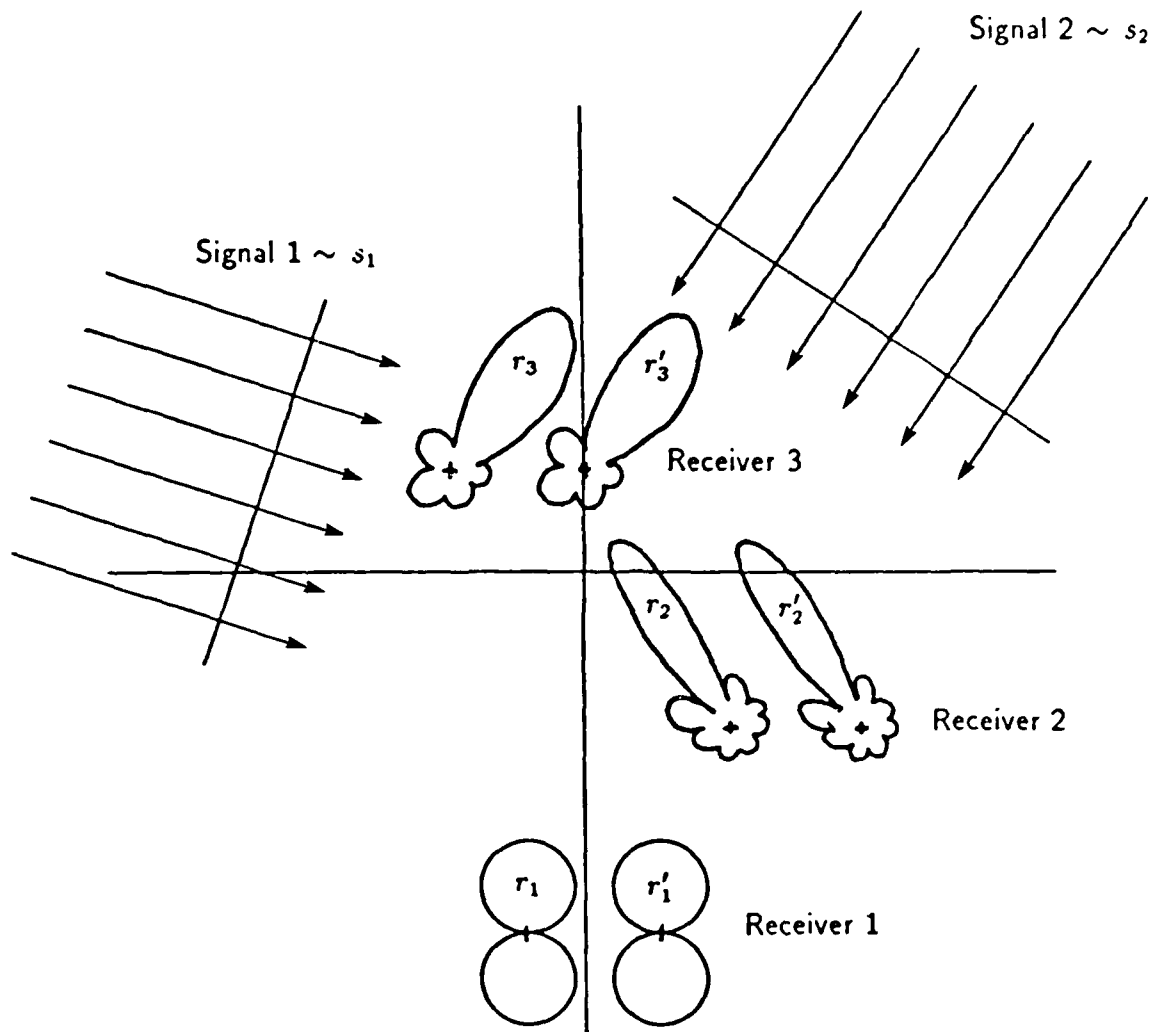
R. ROY, A. PAULRAJ, AND T. KAILATH

INFORMATION SYSTEMS LABORATORY
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THIS WORK WAS SUPPORTED IN PART BY JOINT SERVICES PROGRAM
AT STANFORD UNIVERSITY UNDER CONTRACT DAAG29-81-K-0057 AND
BY THE OFFICE OF NAVAL RESEARCH UNDER CONTRACT
N00014-85-K-0550.

FIGURE 19
ESPRIT - ARRAY GEOMETRY

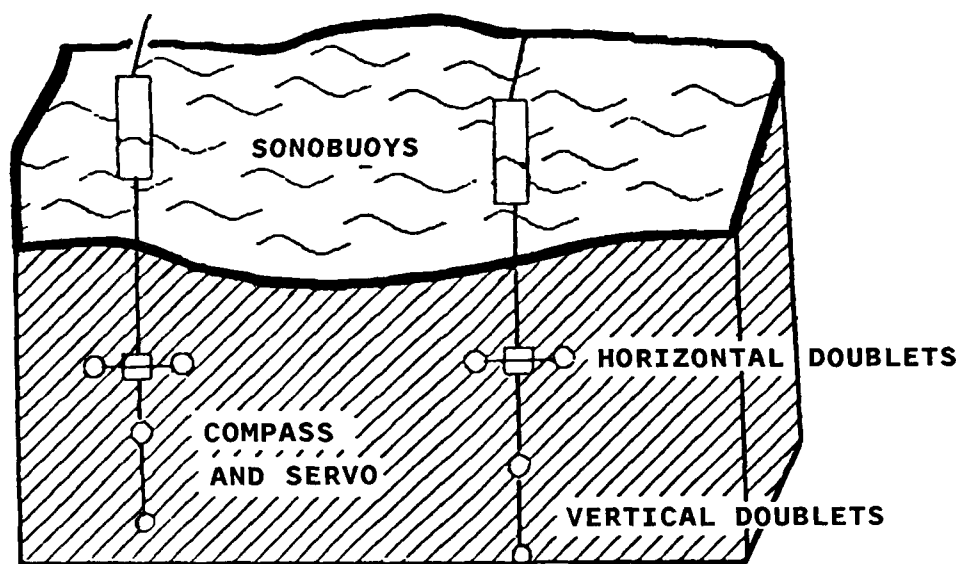


ASSUMING PLANAR WAVEFRONTS AND MATCHED RECEIVER PAIRS WITH THE SAME DISPLACEMENT, THE RECEIVED SIGNALS AND KNOWLEDGE OF THE NOISE COVARIANCE ARE SUFFICIENT FOR ESTIMATION OF THE DIRECTION-OF-ARRIVAL (DOA) AND THE RECONSTRUCTION OF THE SIGNALS INDIVIDUALLY (SIGNAL COPY). NO ASSUMPTIONS ABOUT THE ARRAY GEOMETRY AND SENSOR ELEMENT CHARACTERISTICS, OR THE SIGNAL POWERS AND CORRELATIONS ARE REQUIRED!

the same way, e.g., all to the magnetic north or all vertical. Finally, the separation between the two elements of the doublet must be the same. If one has a situation where these assumptions obtain, then, it turns out, one can reconstruct the signal directions without knowing the array geometry. These arrays could be on buoys floating in the ocean (see Figure 20), or in space (see Figure 21), or on mobile platforms on land, moving around (see Figure 22). The major restriction is that all antenna doublets must be oriented the same way. But then, without knowing the array geometry and without the procedure of having to search at every point, one still can determine the directions of arrival! Moreover, the improvements in time and computation can be quite dramatic (see Figure 23). For a 20-element array covering an arc of 2 radians with 1-milliradian resolution in azimuth and elevation, there is a computational advantage of 10^5 . The computational time on our VAX 11/750 is just a few minutes, as opposed to half an hour using conventional methods or using MUSIC!

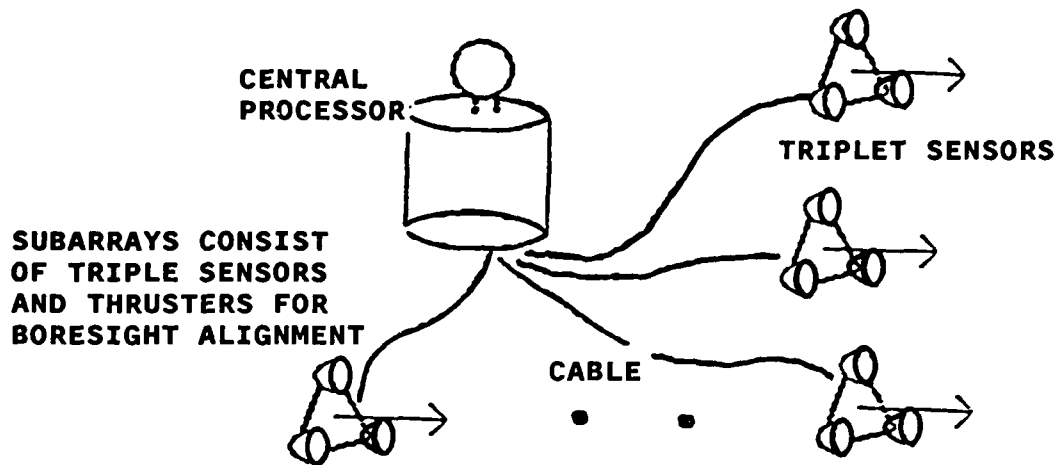
Let me conclude by referring you again to Figure 18, both to note my co-authors, and especially the acknowledgment to JSEP.

FIGURE 20
SONOBUOY ARRAYS



SONOBUOYS ARE AIR-DROPPED AND SCATTER RANDOMLY ON THE SURFACE OF THE OCEAN. CURRENT DOA ESTIMATION TECHNIQUES REQUIRE THAT THE THREE-DIMENSIONAL GEOMETRY OF THE DEPLOYED HYDROPHONES BE DETERMINED--AN OPERATION THAT IS EXPENSIVE AND ALSO UNDESIRABLE SINCE IT OFTEN INVOLVES ACTIVE TRANSMISSIONS WHICH MAY ALERT TARGETS. ESPRIT REQUIRES NO KNOWLEDGE OF SENSOR GEOMETRY. INSTEAD, WE NEED (A) SOME VERTICALLY ALIGNED DOUBLETS TO ESTIMATE DEPRESSION ANGLES, AND (B) SOME HORIZONTALLY ALIGNED DOUBLETS FOR AZIMUTHAL DOA ESTIMATION. THE VERTICAL AND HORIZONTAL ALIGNMENTS CAN BE ACHIEVED VIA GRAVITY AND A MICRO SERVO-MAGNETIC SENSOR, RESPECTIVELY. WITHIN A MINUTE OR SO AFTER THE SONOBUOYS ARE DROPPED, THE NECESSARY ALIGNMENT CAN BE COMPLETED AND ESPRIT CAN PROVIDE DOA ESTIMATES, SIGNAL COPY, ETC.

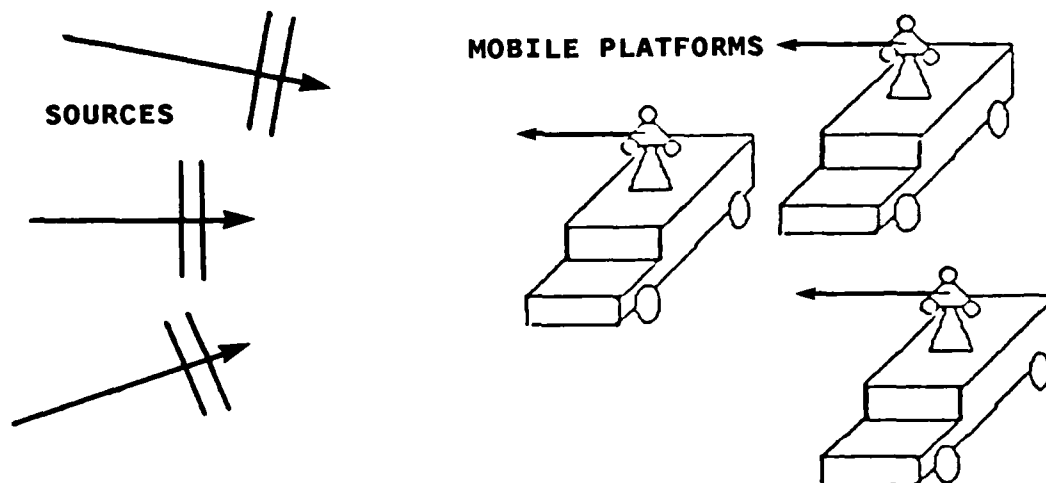
FIGURE 21
LARGE SPACE-BASED ANTENNAS



RIGID, LARGE, AND MULTI-ELEMENT SPACE ANTENNAS ARE DIFFICULT AND COSTLY TO BUILD. FURTHERMORE, IT IS NEARLY IMPOSSIBLE TO CALIBRATE SUCH AN ARRAY IN SPACE. USING ESPRIT, FREE-FLOATING (UNKNOWN POSITION) MATCHED PAIRS OF SENSOR DOUBLETS (OR TRIPLETS FOR AZ-EL LOCALIZATION), WHOSE POINTING DIRECTIONS ARE KEPT ALIGNED BY LOW-COST MICRO THRUSTERS THAT TRACK AN EARTH-BASED BEACON, ARE ALL THAT ARE REQUIRED. SINCE A CONNECTED STRUCTURE FOR THE ARRAY IS NOT REQUIRED, EASE OF DEPLOYMENT AND REPAIR OF SUCH DISCONNECTED ARRAYS CAN HAVE SIGNIFICANT COST AND OPERATIONAL BENEFITS. FOR EXAMPLE, A DEFECTIVE UNIT CAN BE TRANSPORTED TO A SPACE REPAIR STATION OR RETURNED TO THE EARTH.

FIGURE 22

INDEPENDENTLY MOBILE COOPERATIVE PLATFORMS



IN SEVERAL DF APPLICATIONS, MOBILE PLATFORMS ARE PRESENTLY FITTED WITH ESSENTIALLY A SINGLE SENSOR DOUBLET. SUCH A DOUBLET PROVIDES VALID DIRECTIONAL INFORMATION IN A SINGLE TARGET ENVIRONMENT ONLY. ESPRIT CAN USE THE SENSOR DATA FROM SEVERAL SUCH PLATFORMS TO PROVIDE VALID DIRECTIONS IN MULTI-TARGET ENVIRONMENTS. THE PLATFORMS MAY BE MOVING RELATIVE TO EACH OTHER, AS LONG AS THEIR RELATIVE MOTION IS NEGLIGIBLE OVER THE PERIOD NECESSARY TO COLLECT SNAPSHOTS (A FEW MILLISECONDS). NOTE THERE IS NO NEED TO DETERMINE PLATFORM POSITIONS, ETC. HOWEVER, THE DOUBLETS MUST POINT IN THE SAME DIRECTION AND THEREFORE THEY NEED TO BE GYRO STABILIZED. EXAMPLES OF CANDIDATE PLATFORMS INCLUDE AIRCRAFT FLYING IN FORMATION, LAND VEHICLES, ETC. SINCE THE COMPUTATIONAL REQUIREMENTS ARE MODEST, A RETROFIT OF AN ESPRIT-BASED PROCESSOR TO EXISTING HARDWARE MAY BE FEASIBLE.

FIGURE 23
IMPLEMENTATION ADVANTAGES OF ESPRIT

COMPUTATION ADVANTAGES

- o THE ESPRIT ALGORITHM DOES NOT REQUIRE A COMPUTATIONALLY EXPENSIVE SEARCH FOR INTERSECTIONS OF THE SIGNAL SUBSPACE WITH THE ARRAY MANIFOLD.
- o THE COMPUTATION REQUIREMENTS OF ESPRIT GROW LINEARLY WITH DIMENSION RATHER THAN EXPONENTIALLY.
- o EXAMPLE--FOR A 20-ELEMENT ARRAY COVERING AN ARC OF 2 RADIAN WITH 1-MILLIRADIAN RESOLUTION IN BOTH AZIMUTH AND ELEVATION, ESPRIT HAS A COMPUTATIONAL ADVANTAGE ON THE ORDER OF 10^5 OVER MUSIC.

ARRAY CALIBRATION REQUIREMENTS

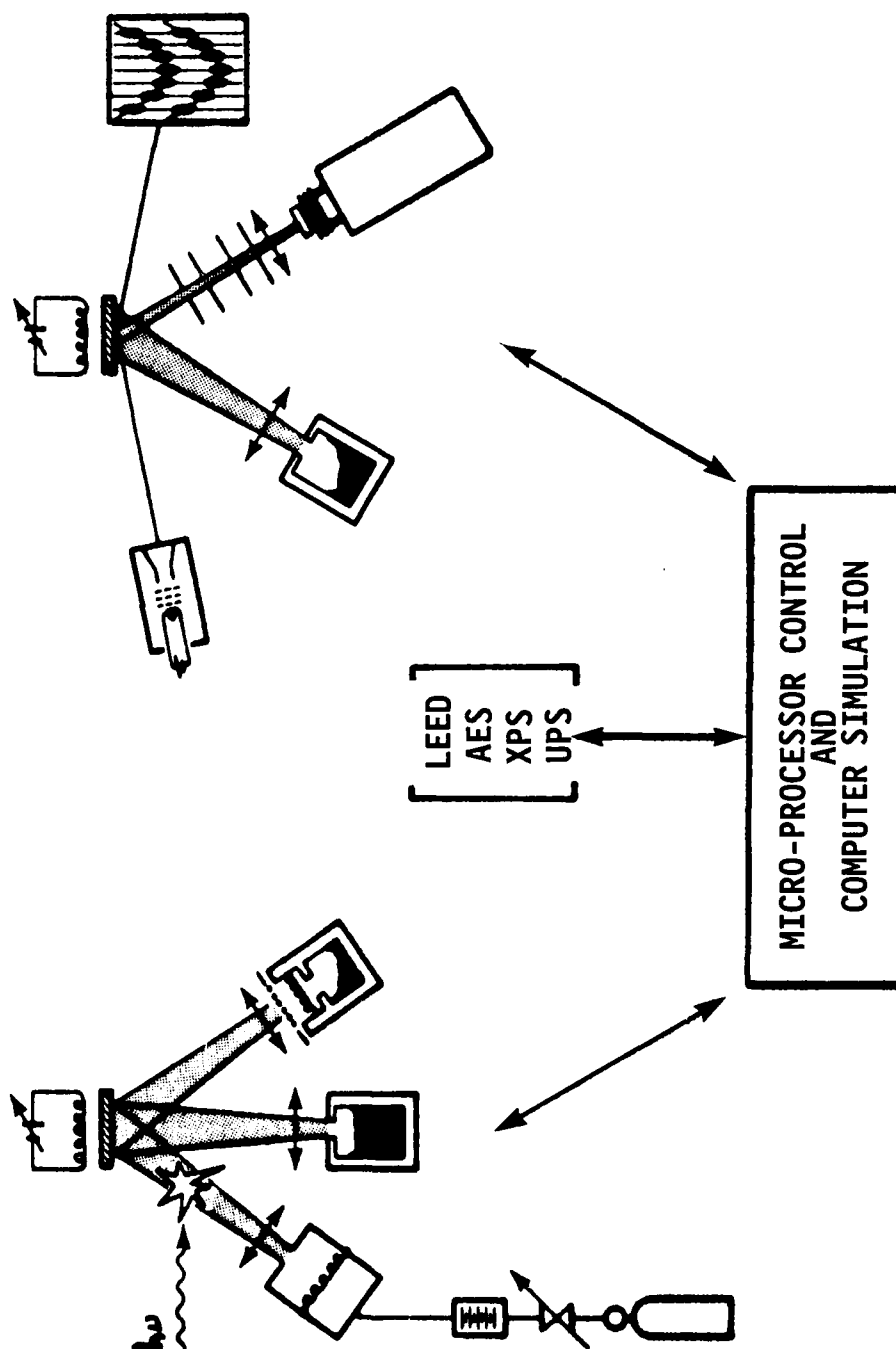
- o ESPRIT REQUIRES NO STORAGE OF THE ARRAY RESPONSE (MANIFOLD) VECTORS FOR ALL POSSIBLE ANGLES OF ARRIVAL. FOR THE EXAMPLE ABOVE, MUSIC REQUIRES APPROXIMATELY 20 MEGABYTES OF STORAGE FOR THE ARRAY MANIFOLD (USING 16-BIT WORDS)--ESPRIT REQUIRES NO STORAGE.
- o ARRAY CALIBRATION IS OFTEN AN EXPENSIVE PROCEDURE AND IN SOME CIRCUMSTANCES IMPOSSIBLE TO PERFORM. ESPRIT'S ABILITY TO WORK WITHOUT ARRAY CALIBRATION IS ATTRACTIVE IN APPLICATIONS SUCH AS SPACE ANTENNAS, SONOBUOYS, ETC., WHERE THE ARRAY GEOMETRY MAY NOT BE KNOWN.

*Electronic Materials Research in JSEP:
The Key to Future Device Technology*

Dr. Joseph Greene
Professor
Coordinated Sciences Laboratory
University of Illinois

Until relatively recently, progress in the development of new device technologies has really been limited by the rather slower rate of progress in our ability to fabricate new materials, to optimize materials properties, and to develop new materials processing technologies. What I would like to try to do is to convince you that this imbalance is changing rapidly. This is due in no small part to funding of the Joint Services Electronics Program across the country. In fact, this change is so dramatic that in many areas, the immediate results of fundamental research in materials, solid-state physics, and surface physics are now driving device evolutions in very sophisticated quantum-based technologies that we would not have been able to imagine just a few years ago. This has come about in great part because of our ability to understand and to do chemistry at the atomic level, essentially atom by atom. That ability itself has come about because of the confluence of a number of fields of physics and engineering. One example is vacuum technology itself, the ability to build vacuum systems that will pump and recycle rapidly to ultra-high vacuum with load locks. Another is the ability to specify the chemical state and the energy of incident particles at the substrate during crystal growth. What I have tried to illustrate in cartoon fashion (Figure 1) is that not only can we use effusion cells (the normal evaporation cells in molecular beam epitaxy systems), but also we have the capability for using gases by passing them into a cell to crack them and produce radicals. Alternatively, we can have an effluent beam coming in, which when hit with a laser beam produces vibrationally or electronically excited radicals having very different reactivities with the surface than do molecular beams. Furthermore, we can use energetic ionized

FIGURE 1
CRYSTAL GROWTH IN ULTRA-HIGH VACUUM
CHEMISTRY AT THE ATOMIC LEVEL



species that also react very differently with the surface. In this way the incorporation probability of species can be changed by many orders of magnitude. It is really possible to do chemistry at the atomic level. Of course, if you want to control the incoming beam species you also have to control the environment of these species as they condense on the solid. That means that you must have very well-characterized surfaces. They must be completely clean, and one must know in exact detail the positions of atoms on these surfaces. We can do that in ultra-high vacuum in the same system in which growth occurs. I represent this using electron diffraction as one of many possible probe types that produce visible patterns. A key feature of these sophisticated tools is that people really are able to do very high quality physics. We understand in many cases what is going on at the atomic level well enough so that we can do computer simulation and begin to predict the sort of experiments that one really ought to do. This is a complete turnabout to what people thought about in the area of crystal growth technology ten years ago.

The reason for this advancement is that crystal growth is really a combination of a lot of very sophisticated fields. No one piece of information is really enough to make rational decisions as to how one should do experiments. It is a consortium of surface and thin film physics, solid-state chemistry, and a number of other fields (Figure 2). Just to give you some idea of what one needs to know, let me point out that it is not enough to know just the crystal structure of a silicon or a gallium arsenide substrate, because the crystal reconstructs at the surface. It costs energy to break any solid material. That

FIGURE 2
CRYSTAL GROWTH

- o NUCLEATION KINETICS (SURFACE PHYSICS)**
 - SURFACE STRUCTURE; RECONSTRUCTIONS
 - STICKING PROBABILITIES
 - SITE-SPECIFIC BINDING ENERGIES
 - ADSORBATE-ADSORBATE INTERACTIONS
 - NUCLEATION MECHANISM (2-D/3-D/S-K)

- o GROWTH KINETICS (THIN FILM PHYSICS)**
 - POINT DEFECTS
 - DISLOCATIONS, TWINS, STACKING FAULTS
 - ANTIPHASE BOUNDARIES
 - GRAIN BOUNDARIES

- o ELEMENTAL INCORPORATION PROBABILITIES**
(SOLID-STATE CHEMISTRY)
 - DESORPTION KINETICS
 - SEGREGATION
 - SOLID SOLUBILITY
 - CLUSTERING, ANTI-SITE FORMATION

energy is really the energy to create two new surfaces. These surfaces are really defects. The surface atoms rearrange their bonds to minimize their energy. In gallium arsenide alone there are tens of these reconstructions to understand and predict. It is not enough to develop a stochastic number for the probability of some reaction. One has to know where each atom is going to go and what it is going to do. If one could predict all of these factors, one could say something about how films should grow. Moreover, one could then control the structure, and hence, the electro-optical properties of these materials.

Let me say a few words about anti-phase boundaries. Currently, these are now of much interest to people who wish to grow gallium arsenide on silicon. Figure 3 is a schematic of one of the types of growth systems that our group uses, a modified MBE machine. Samples are inserted through a load-lock and transferred to the growth chamber, which contains several beam sources (examples are the effusion cell as well as low-energy ion guns that we designed in a JSEP program and are now available from manufacturers). The sort of thing that one wants to do is to pick a surface, such as silicon (100) surface, then look at various materials absorbed on that surface. I will show you some examples for both group III and group V atoms. One can do III's and V's on silicon (including gallium arsenide on silicon). Figure 4 illustrates some of our very recent work. The figure shows a surface phase diagram that is a map; it tells you what you should expect for the detailed structure of atoms on a well-defined, clean surface. This is a 2×1 surface at one low range of temperature. As you put a few atoms on, they act as a two-dimensional gas; each

FIGURE 3

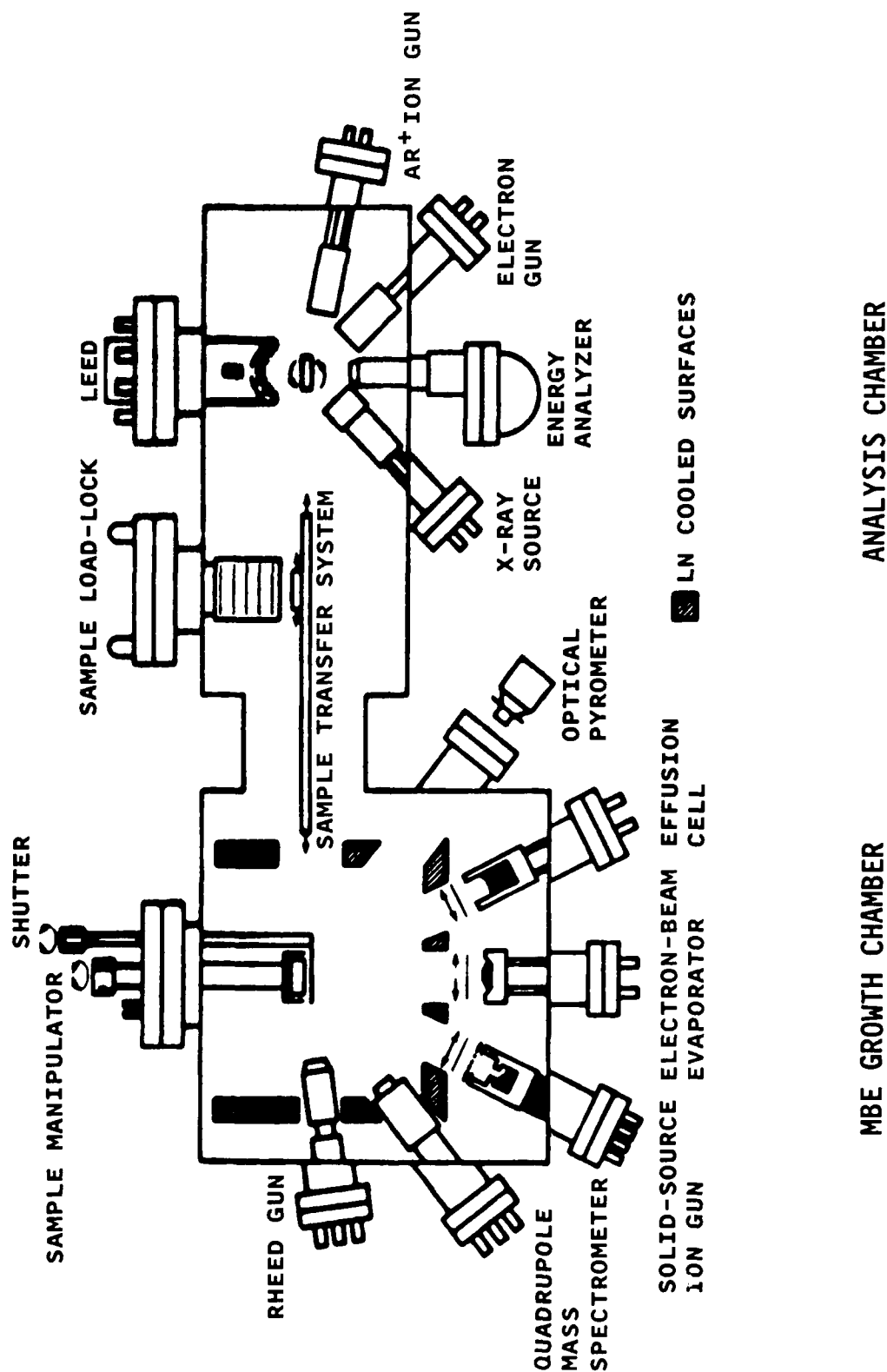
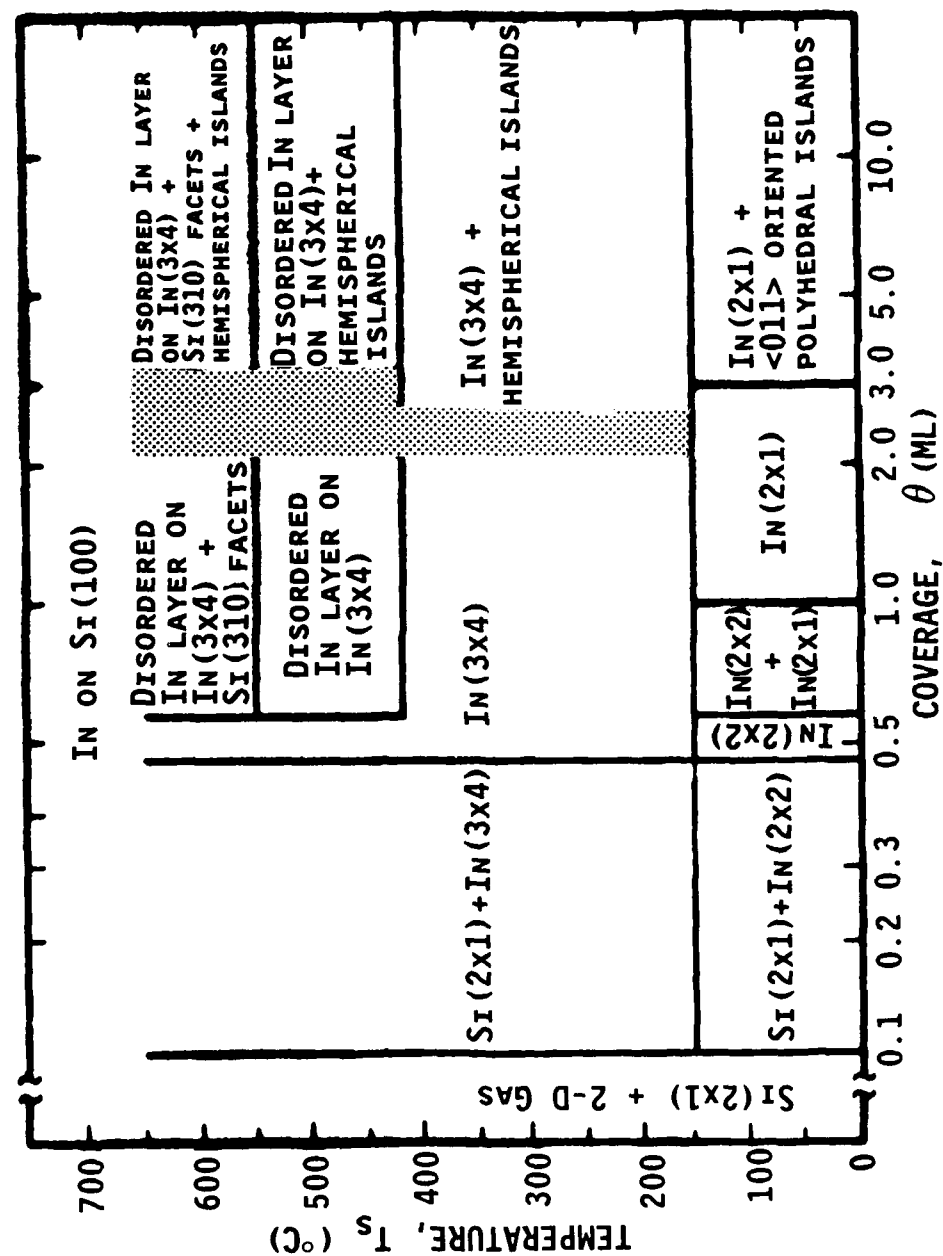


FIGURE 4



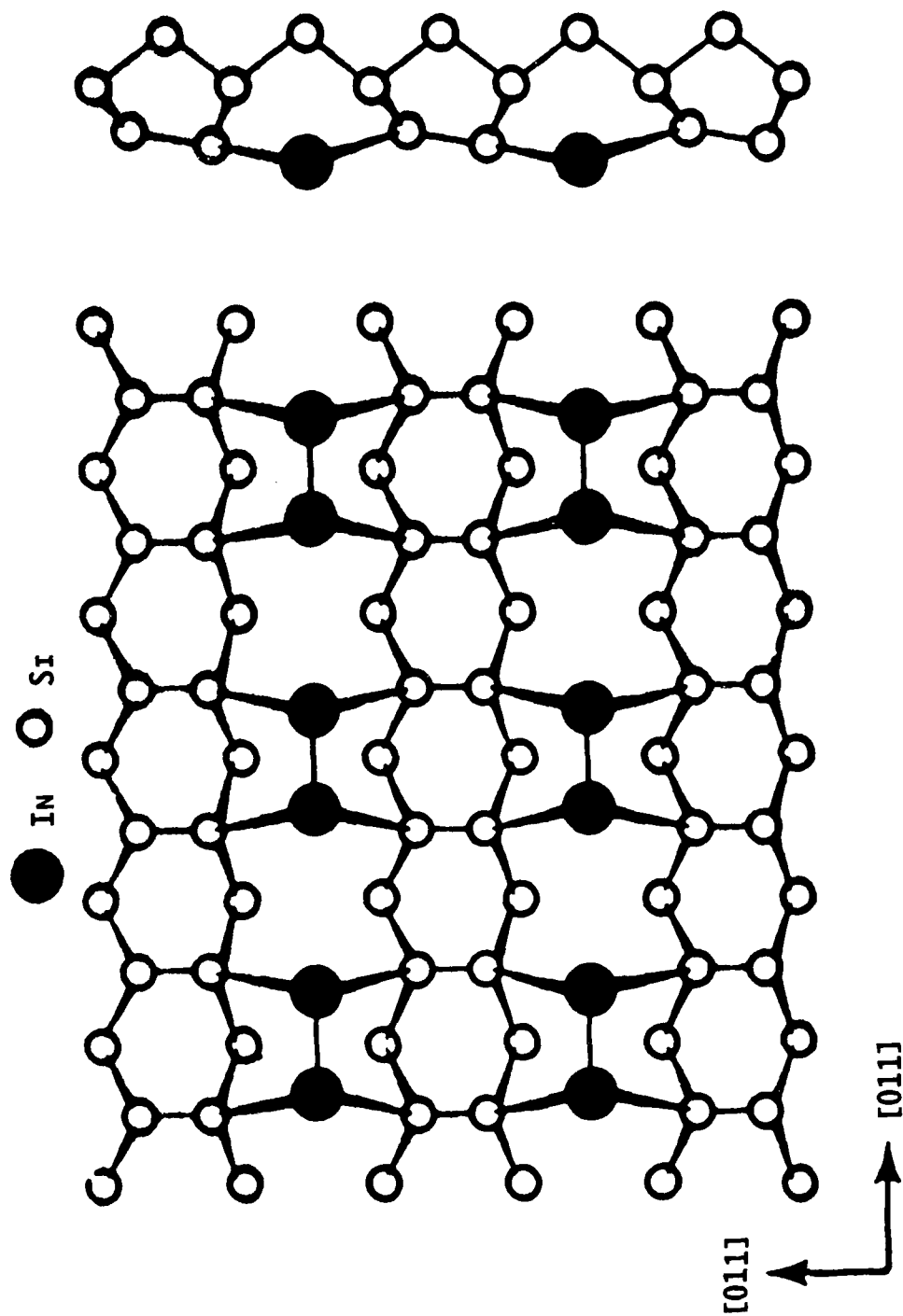
KNALL, SUNDGREN, HANSSON, GREENE, SURFACE SCIENCE, 512 (1986)

atom is unaffected by the others. Eventually they condense into 2×2 islands. This nomenclature indicates translational vectors that are integer numbers of the unit translational vector of the perfect lattice. So a 2×2 means there is an atom at every second site of the bulk lattice. Eventually you get a full monolayer, and as you continue you get other surface structures. Just to show you one practical application of that, if we change the surface structure and grow an indium film, what you get is completely different, depending on what structure you start with. Something that is really startling is the case when 200 monolayers of indium are deposited on a silicon (100) 2×1 surface. In this case less than 7 percent of the surface is covered and the indium nucleates in only one direction into one-dimensional wires. You need no lithography to do that; all you need is to understand some basic surface physics. Figure 5 shows a 2×2 structure in the (100) plane. The silicon atoms are in a tetrahedral array. In the 2×2 indium structure the indium is present as dimers along channels. These are more evident in the side view. Once an indium atom hits a channel, it costs energy to jump to an adjacent channel, so the easiest diffusion direction is along the channel. There is very little growth sideways; the growth is down the channels in one dimension. If you pick another surface, such as the 3×4 surface, which does not have these sort of channels, you get the classic metallurgical textbook behavior of three-dimensional nucleation.

In addition to information about the structure, what you would really like to have is information about how these atoms bind with the surface. You would like to know where they sit, and also the electron distribution. What

FIGURE 5

Sr (100) 2x2 -In



is the binding energy in the various sites? In addition to doing diffraction, what you can do is have a beam source hit the surface (Figure 6), and you can chop the beam coming in and chop the beam going out in order to look at the phase relationship between the incoming and outgoing particles. If you pick a group V species like antimony, and evaporate that, you get antimony tetramers (groups of four atoms) coming off. You learn from the mass spectrometer that you are doing chemistry, whether you like it or not. If you look at the temperature range (Figure 7) at low temperature you get antimony-4 coming off; at high temperatures you get antimony-1. Unfortunately, antimony is the most common donor dopant in MBE silicon, which normally grows at about 700 degrees or so, and you see that the desorbing species at that temperature is both antimony-1 and antimony-4. That means if you want to model the incorporation of this dopant, you are going to have to know something about the physics, and how it desorbs, because that will determine how much dopant is incorporated. One can understand this behavior--I will explain it using the cartoons in the figure. At low temperatures these tetramers have a unity probability of reacting with the extremely catalytic silicon surface. These tetramers are dissociatively chemisorbed, and form a monolayer--the binding energy is very strong (nearly 2 and 1/2 eV). Also, antimony does not bond to antimony at all at low temperatures, so as it hits the surface it moves along and it desorbs. That is the antimony-4 that you see. At very high temperatures antimony-1 begins to come off, since even a single atom can get enough thermal energy to come off. Here you see primarily antimony-1, but you can never get a full monolayer coverage. As a consequence, you always have some open surface. At

FIGURE 6

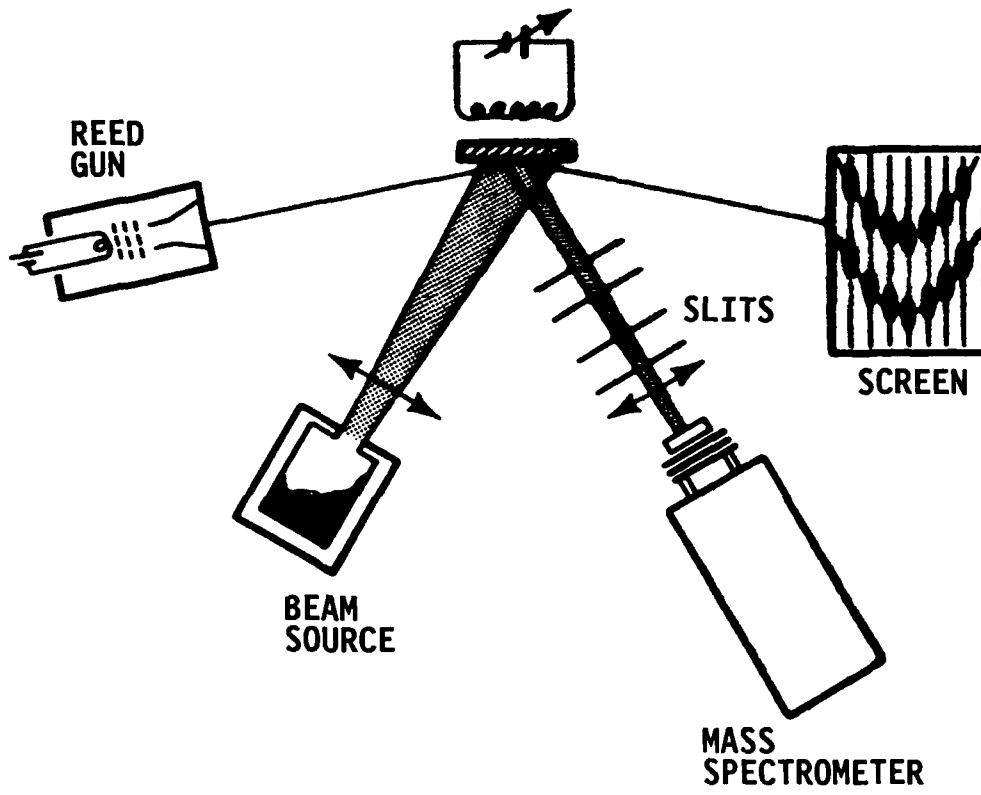
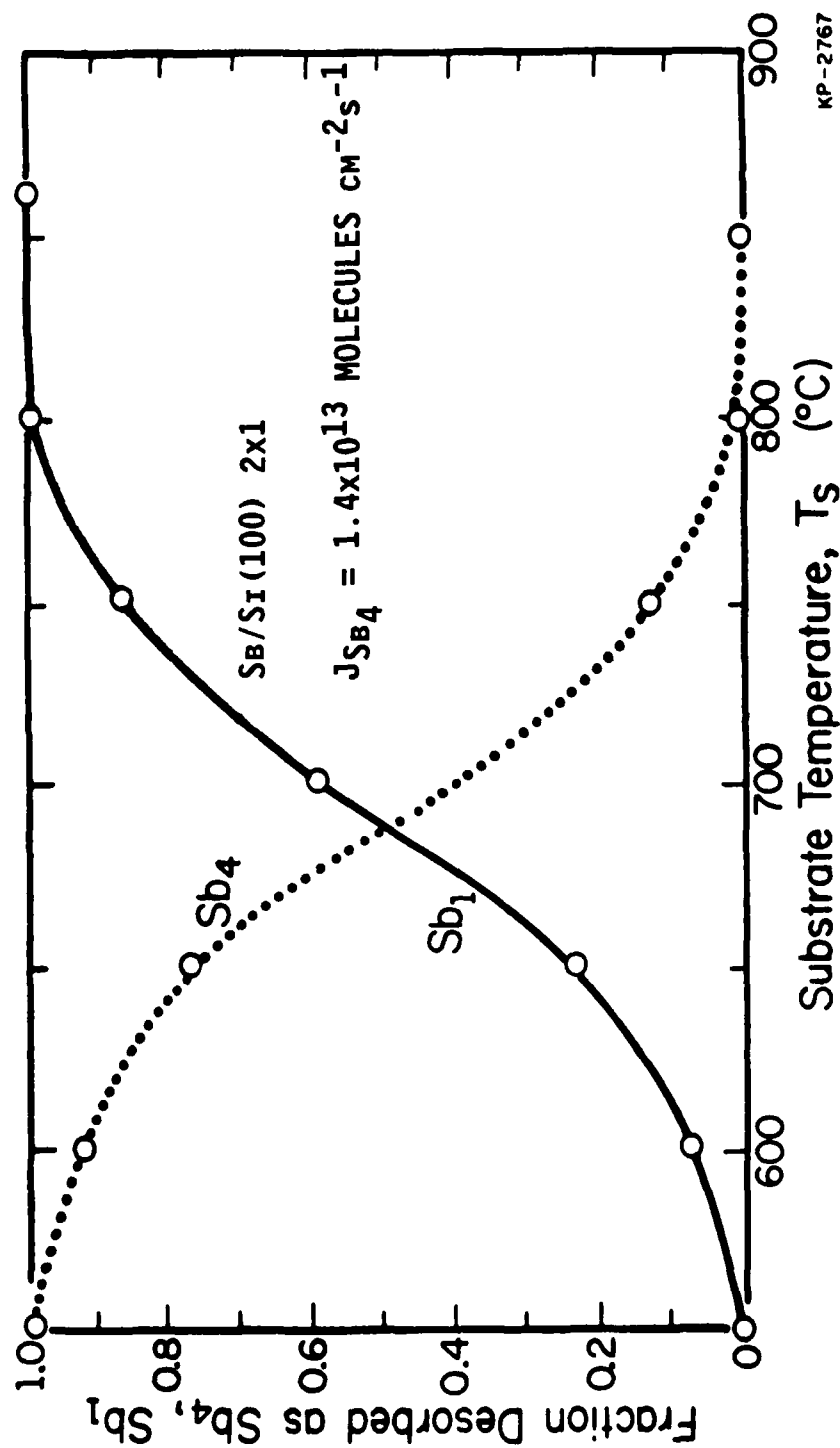
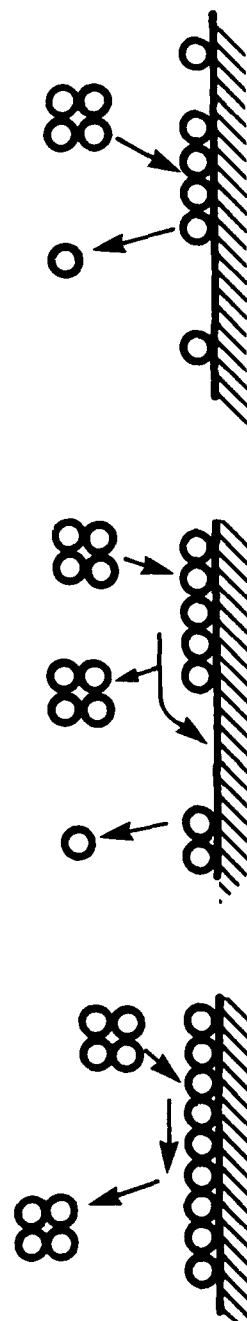


FIGURE 7



KP-2767



intermediate temperatures the physics is interesting, because now you have antimony-1 coming off in addition to antimony-4 coming down. It is now a game of kinetics--can the antimony-4 get to the open spot on the surface, in which case it will adsorb? If not, it will desorb. That is why you get both types coming off.

As I said, you would like to know the binding energy, and you can get that by using the phase relationship. In Figure 8, I have picked a high temperature, so we only have one species, antimony-1. I am allowing one hundredth of a monolayer, (very few atoms per square centimeter per second) to come down. I turn the beam flux on and detect the flux coming off with the mass spectrometer. The flux continues to rise exponentially until you reach steady state. One can fit the exponential rise to something that goes exponentially with time, divided by the lifetime on the surface. Of course, the lifetime of the atom on the surface is directly related to the binding energy. When you turn the beam off, the flux goes down exponentially and these two kinetics should be the same. Another piece of information is obtained from the shaded area. The integrated area between the desorbed flux and the steady state level is the material left on the surface, or the saturation coverage for that temperature. At 825°C it is about one-third of a monolayer. That also is directly related to the binding energy. So by putting these two pieces of information together you can deduce the binding energy, as shown in Figure 9. This is antimony on silicon (100), as a function of coverage up to one monolayer. The binding energy is about 2.4 e.v. up to about one-half monolayer coverage, and then it switches to about 0.7 eV less. This small difference in

FIGURE 8

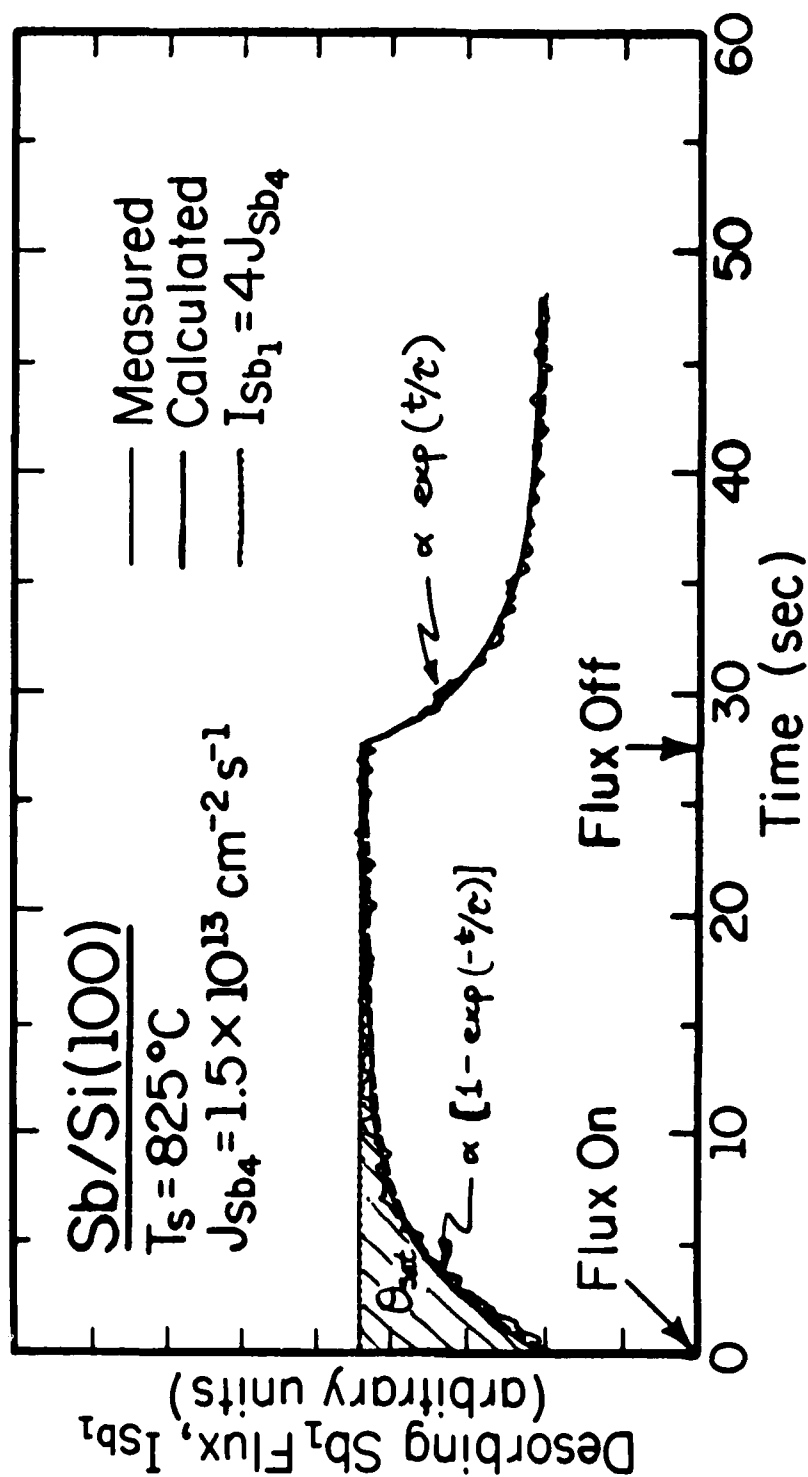
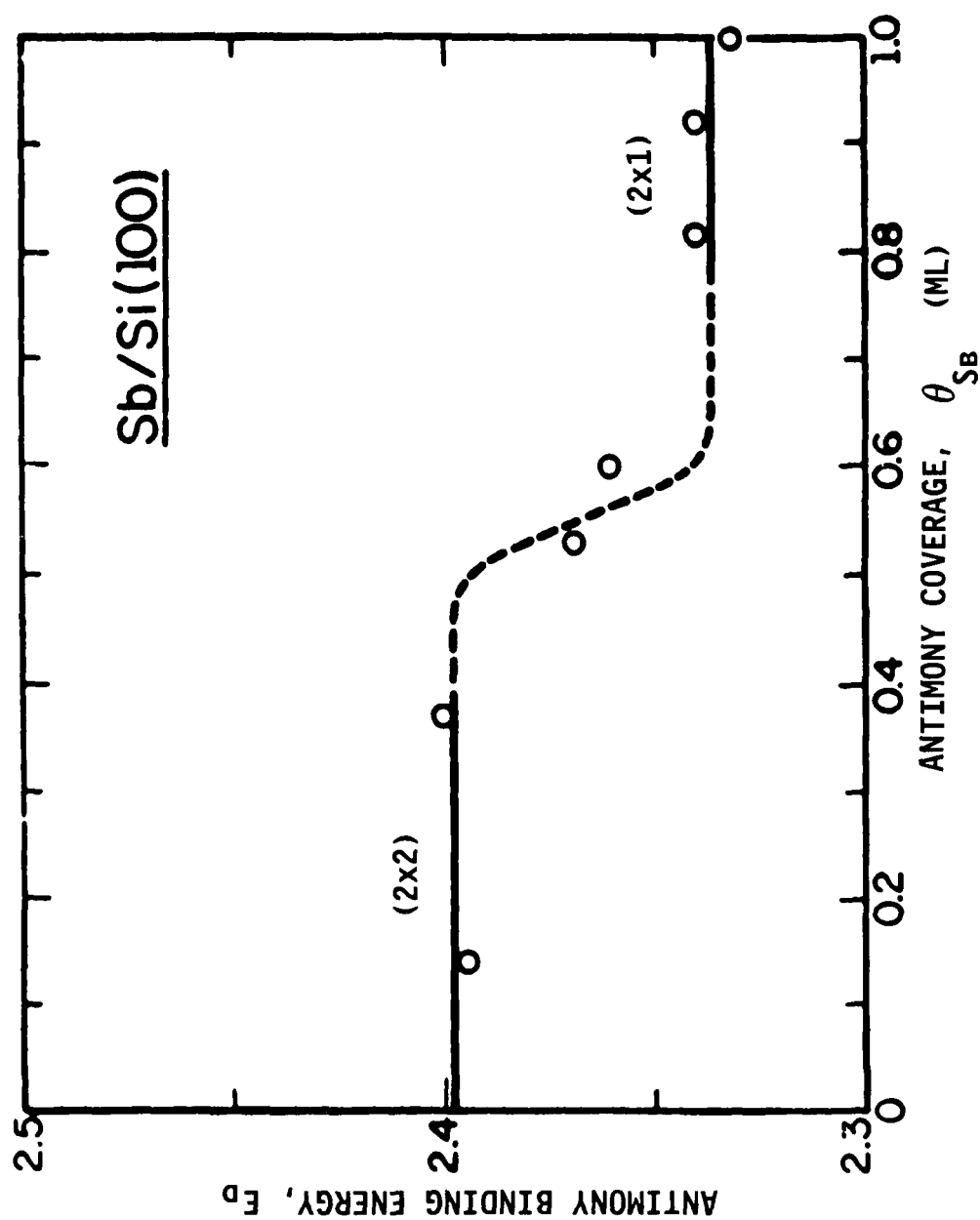


FIGURE 9

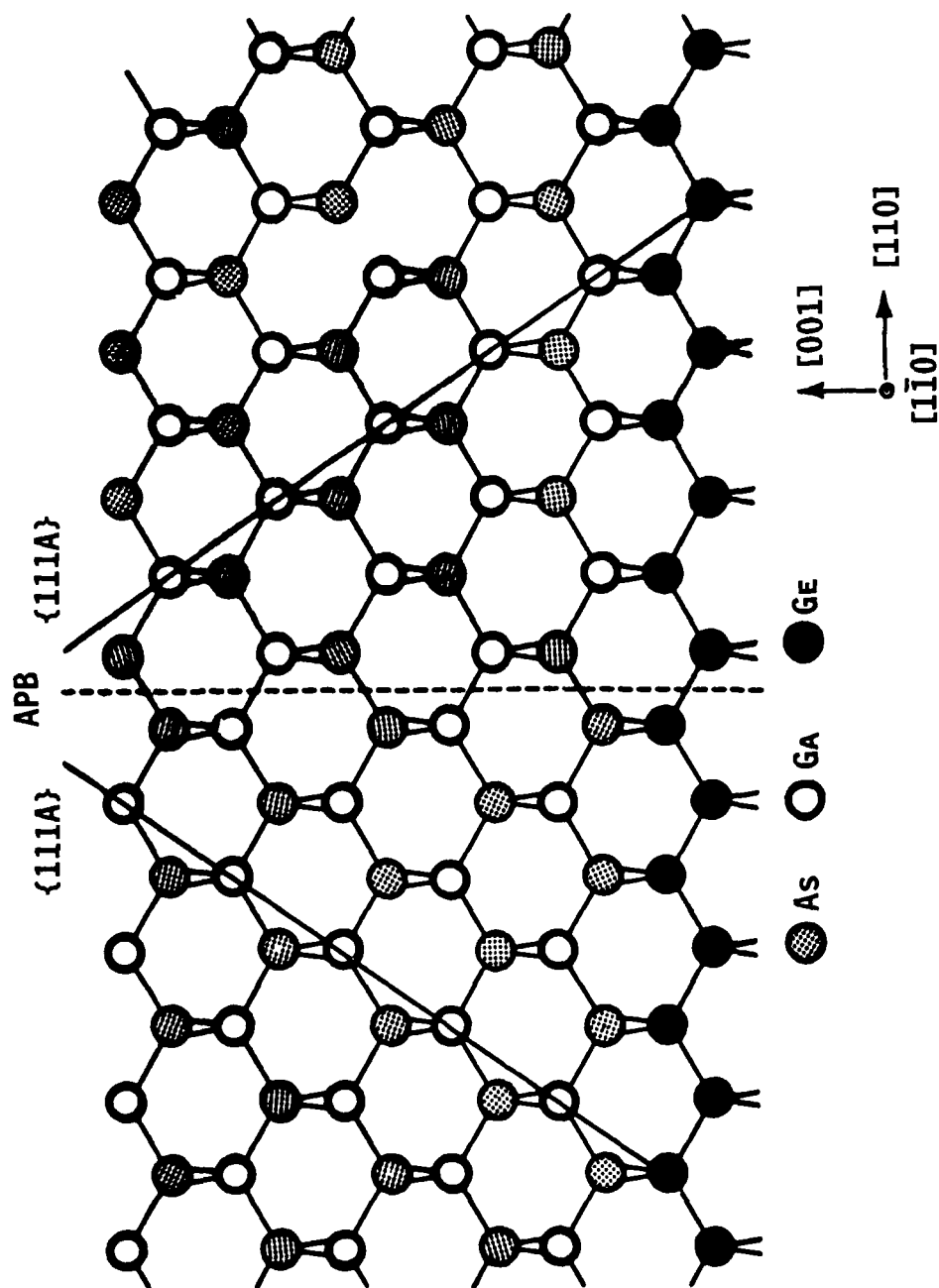
Sb₁ BINDING ENERGY E_D TO THE Si(100) SURFACE AS A FUNCTION OF ANTIMONY COVERAGE θ_{Sb}



energy is extremely important because everything goes exponentially as the energy divided by kT . This is due to structural phase transition from 2×2 to 2×1 .

There are a lot of ways that one can use this information. I want to give two examples. The first is from Haddis Morkoc at our laboratory and the second is from our own work. People have been wanting to grow gallium arsenide on silicon for a long time. This is not a new idea, although it recently has become very popular. The reasons for wanting to do this are multifold. One is the economics involved (certainly silicon substrates are much cheaper than gallium arsenide), but the real driving force is probably that one wants to integrate opto-electronics with silicon integrated circuitry. In any case, there are grave material problems in reaching that goal. It is not just that there is a large lattice constant mismatch or thermal expansion mismatch leading to stress, but also there is a more significant and fundamental problem that has to do with the fact that even though the lattices of gallium arsenide and silicon are isostructural, in the sense that they are both phase centered cubic lattices with two atom bases, there is a difference. In gallium arsenide those two atoms are different, one is gallium and one is arsenic, but in silicon they are the same. That leads to a fundamental problem illustrated in Figure 10, that is, if we have a silicon (100) surface, represented by the dark colored balls and we have gallium and arsenic atoms depositing on the surfaces, they cannot communicate with each other through the silicon lattice because there is no sublattice in the silicon. The atoms can start at a particular spot but as they grow together there is a high probability that when they come

FIGURE 10



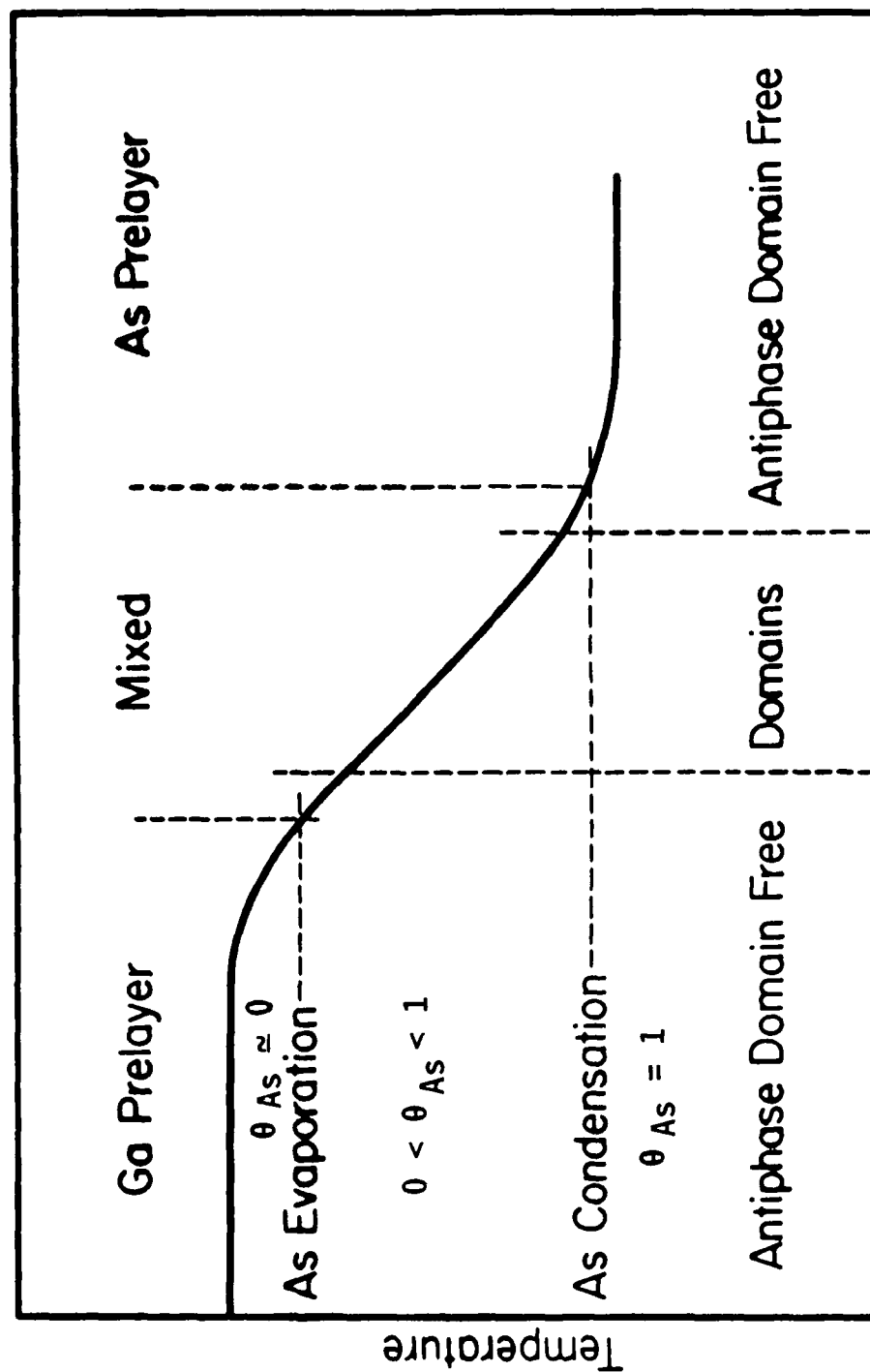
FISHER, MORKOO, NEUMANN, ZABEL, CHOI OTSUKA, LONGERBONE, ERICKSON, J. APPL. PHYS. 60, 1640 (1986).

together they form arsenic-arsenic bonds and gallium-gallium bonds. These anti-site bonds form what is called an antiphase boundary, a region of very high local charge that gives rise to scattering.

How does one attack that? Figure 11 shows that, just as in the case of antimony on silicon, there are certain critical temperatures for the case of arsenic on silicon. Below what Haddis Morkoc has called the arsenic condensation line, if one has only an arsenic flux on to the silicon surface one will get an arsenic monolayer. If one approaches that critical temperature, no excess arsenic exists because the arsenic-arsenic bond is much less than the arsenic-silicon bond. In between that situation and what he has called the arsenic evaporation line one gets a coverage between zero and one. Above that point nothing sticks. One might attack this problem by making the silicon lattice think that there really are two substrates here, i.e., by filling the surface with a full arsenic monolayer. Although this seems obvious, nonetheless, one must know a little about the physics to be able to do that.

Figure 12 shows how anti-phase boundaries can result from these steps. If one studies the lattice in detail it is apparent that by a tilting the surface more than 2 degrees from the (001) direction toward a (100) direction, one can derive steps that are half a lattice spacing (double-size) steps. As shown in Figure 13, these double steps can eliminate anti-phase boundaries. That is one example of using an understanding of the surface in order to develop new technologies.

FIGURE 11



MORKOQ, OTSUKA, ZABEL, SCIENCE, IN PRESS.

FIGURE 12

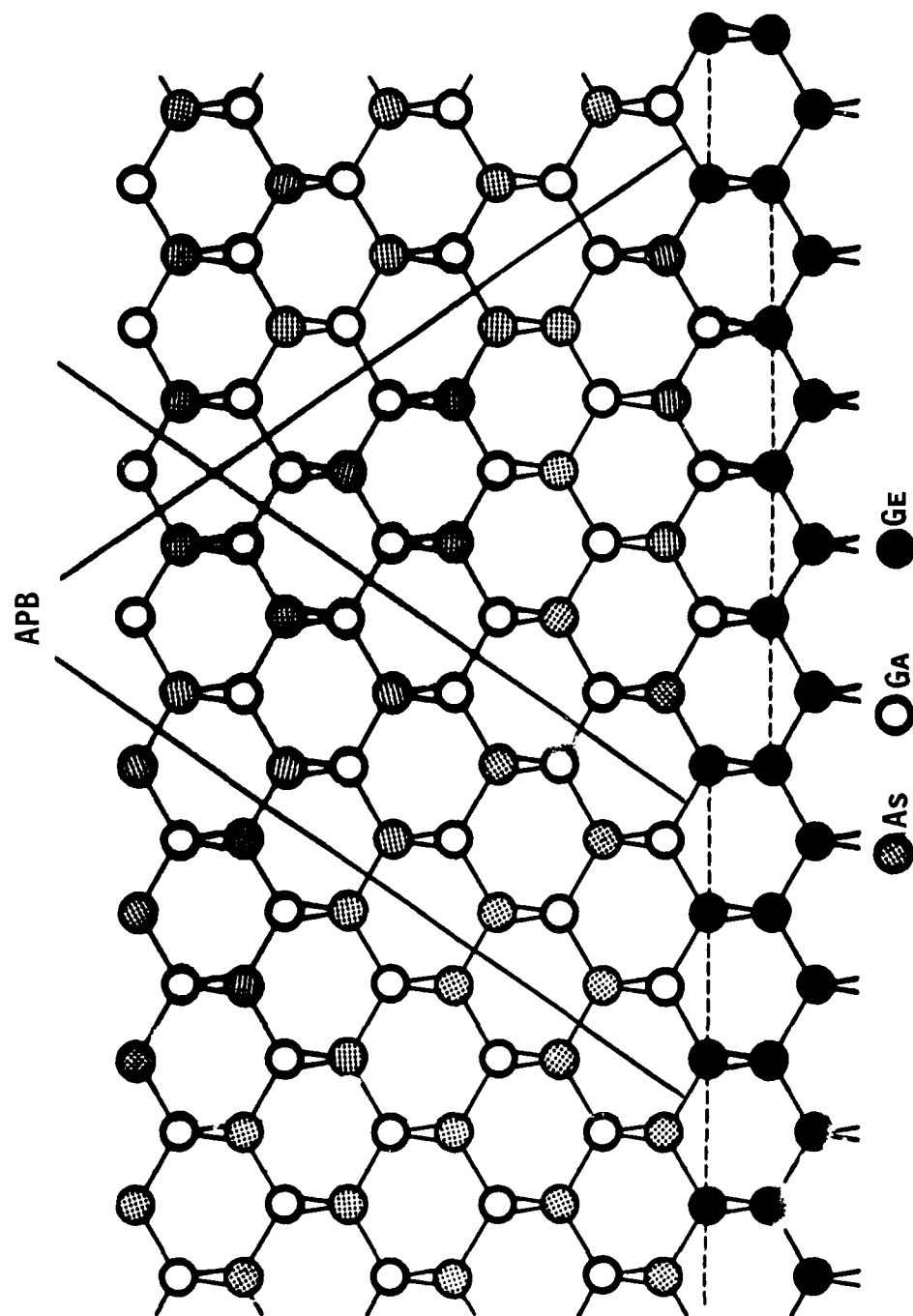
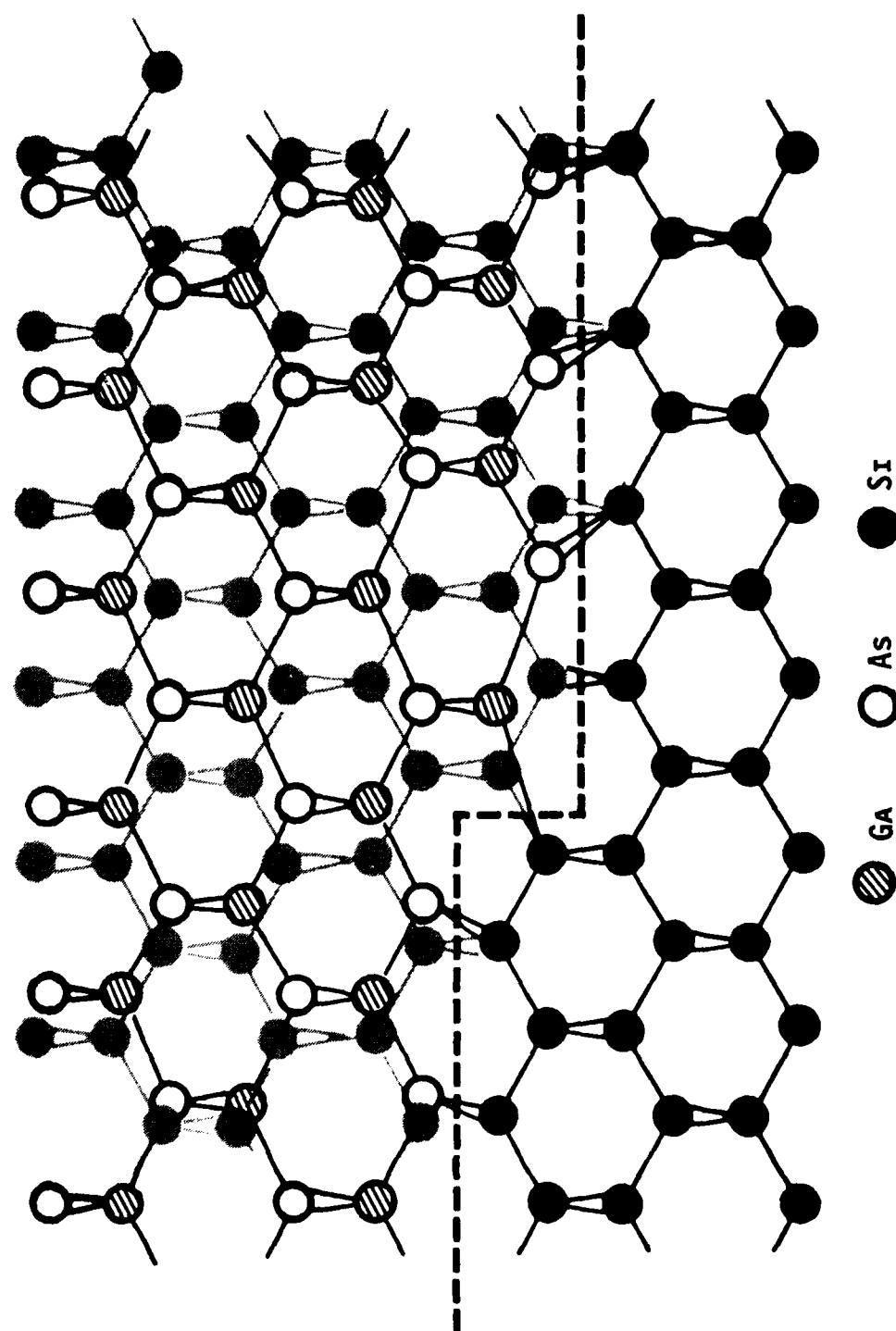


FIGURE 13



Another important result is that in order to use quantum-level devices in which we observe quantum modulations, we really need to control the dopant as well. In general, one would like to find throughout the film, or even in a modulated region, a uniform concentration of dopant, as represented by the dotted line in Figure 14. That situation is never what you get. In fact, in gallium arsenide, as well as in silicon MBE, there really are very few dopants that behave anywhere near ideally. What happens in almost all cases is that you get a depletion region before you reach the steady state, which can be several thousand angstroms thick. The excess atoms are at the surface, where enhancements of four or five orders of magnitude are possible. One of the worst cases is that of antimony on MBE silicon, where at normal growth temperatures one can easily get full monolayers on the surface. That tells you that you are not growing on silicon anymore; you are growing on antimony.

An indication that this is really true is given in Figure 15, which shows MBE silicon with indium doping. The dotted lines are the desired dopant modulations in the film, which were predicted by simply taking into account known incorporation probabilities. The experimental results differ greatly from these profiles. Depletion regions are evident at the bottom of both doped layers. Also, there is a huge concentration of indium at the surface, and the pile-up of indium at the surface of the doped layer acts as a reservoir for doping the center (nominally undoped) layer. The bulk concentration is less than about ten parts per million, yet the surface gets so saturated with indium that you really are growing on indium rather than

FIGURE 14
DEPLETION/ACCUMULATION

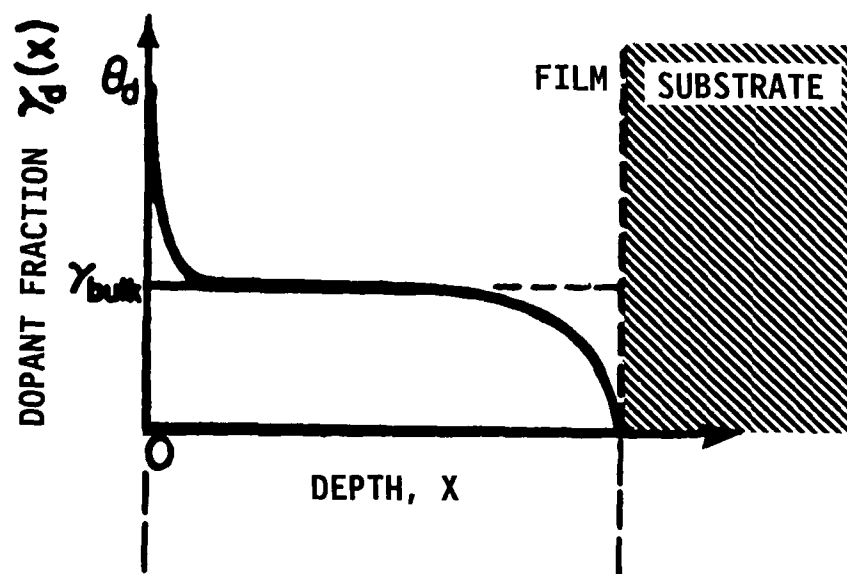
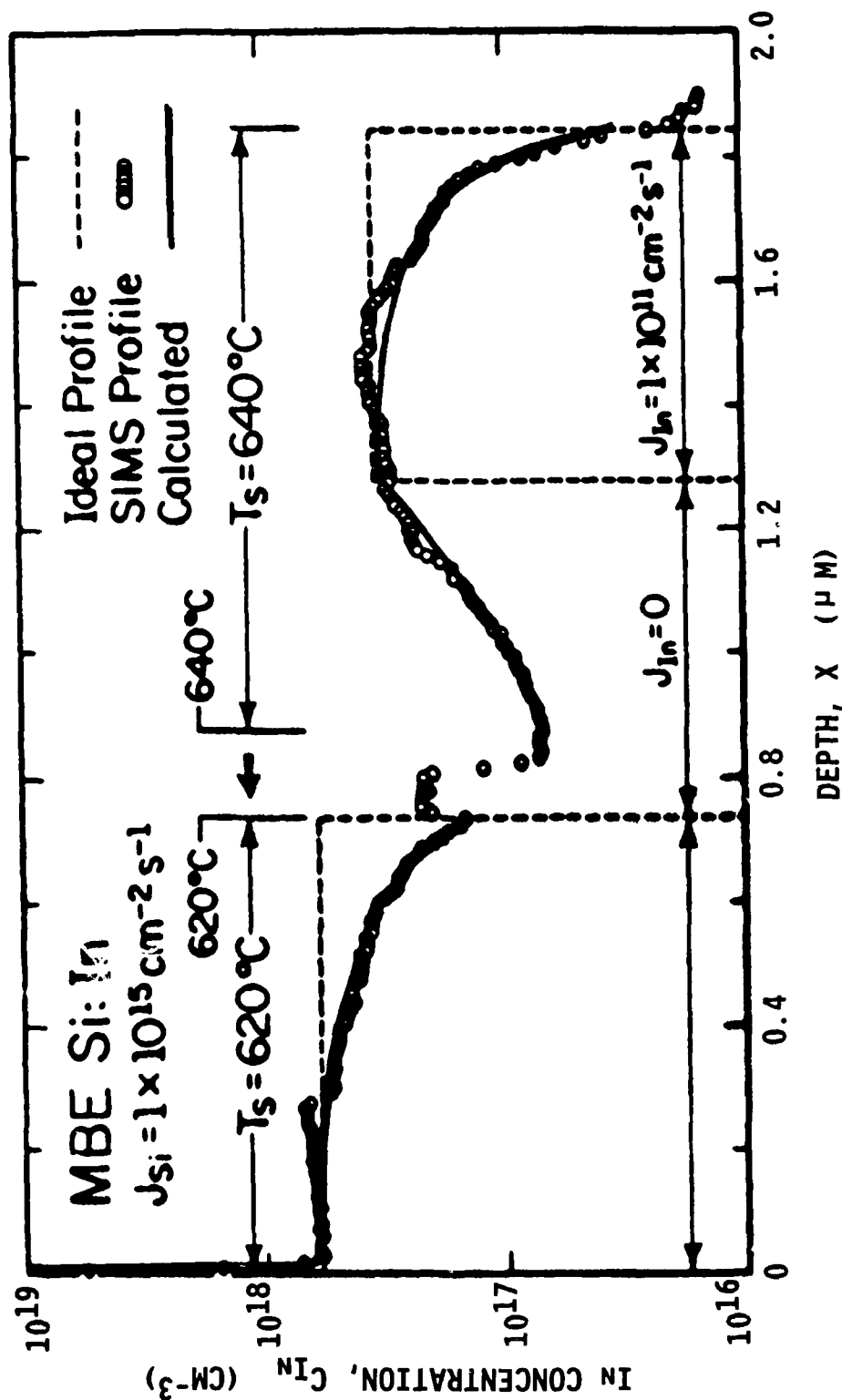


FIGURE 15

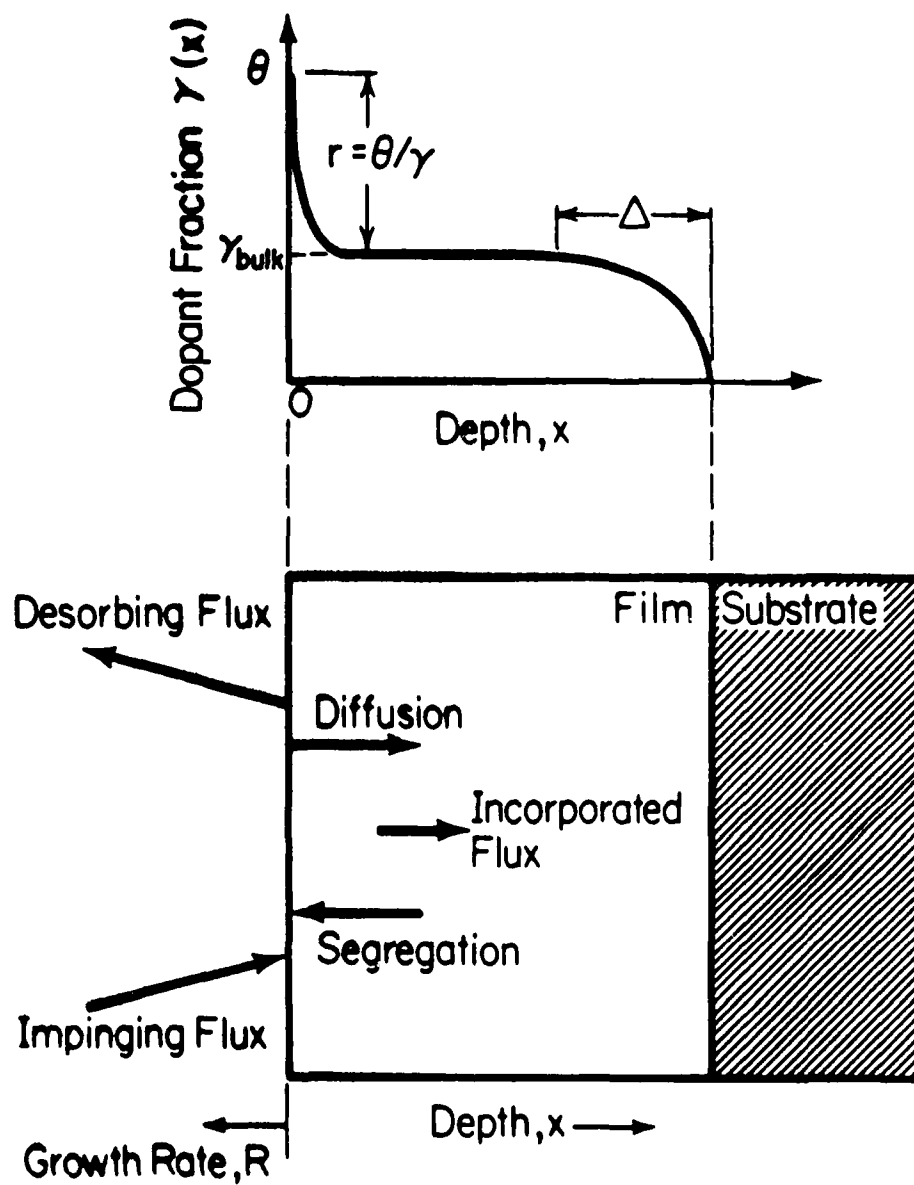


ROCKETT, BARNETT, GREENE, KNALL, SUNDGREN, J. VACUUM SCIENCE AND TECHNOLOGY A3, 855 (1985)

silicon, and this gives rise to deep level defects. We have modeled this effect by including factors shown in Figure 16, and calculated dopant profiles in both gallium arsenide and silicon. Once you have a model, you can then turn it backwards and ask what can you do well. If you can change the structure, if you can change the chemistry of the growing film, certainly you can develop new materials, such as those in Figure 17. Various other new electronic materials also have come out of this kind of research.

I would like to end with a discussion about devices. The theme of this talk is to move from knowing something about surface and solid-state physics to developing devices. Hot electron physics has been hot for a long time. A number of people in this room participated in the field and have been very interested in what happens if you accelerate electronics to high fields. Karl Hess at the University of Illinois, as far as I know, was the first one really to exploit this to make new devices (Figure 18). These are called real space transfer devices, a new class of devices that I will briefly describe. The most recent conference on hot electrons and holes in semiconductors uses as their logo a picture taken directly from Karl Hess's early papers. The real space transfer device (Figure 19) operates on many of the same principles that quantum well devices operate. The structure (Figure 20) consists of an alternating set of thin layers, e.g., aluminum gallium arsenide/gallium arsenide. The figure shows the difference in conduction band edge for these two when the aluminum gallium arsenide is doped n-type. The electrons go to their lowest energy level and reside in the undoped gallium arsenide layers. Karl Hess's unique idea was to apply a field

FIGURE 16



GREENE, BARNETT, ROCKETT, BAJOR, APPLIED SURFACE SCIENCE
22/23, 520 (1985)

FIGURE 17
NEW MATERIALS
SINGLE-CRYSTAL THERMODYNAMICALLY
METASTABLE SEMICONDUCTORS

$(\text{GaAs})_{(1-x)} (\text{Ge}_2)_2$	}	ISOSTRUCTURAL
$(\text{GaSb})_{(1-x)} (\text{Ge}_2)_x$		NON-ISOVALENT
$\text{Ge}_{1-x}\text{Sn}_x$	}	ISOVALENT
$\text{InSb}_{1-x}\text{Bi}_x$		NON-ISOSTRUCTURAL
$(\text{GaSb})_{(1-x)} (\text{Sn}_2)_x$	}	NON-ISOSTRUCTURAL
$(\text{GaSb})_{(1-x)} (\text{Ge}_2(1-y)\text{Sn}_2y)_x$		NON-ISOVALENT

CHARACTERIZATION:

- o STRUCTURAL: XRD, TEM, CHANNELING, EXAFS
- o ELECTRICAL: HALL, C-V
- o OPTICAL: ABSORPTION, ELLIPSOMETRY, XPS
- o THERMODYNAMIC: DSC, DTA
- o LATTICE DYNAMICS: RAMAN
- o THEORY: ELECTRON AND PHONON BAND STRUCTURE

GREENE, CRC CRITICAL REVIEWS IN SOLID STATE AND MATERIALS SCIENCES 11, 47 (1984).

FIGURE 18

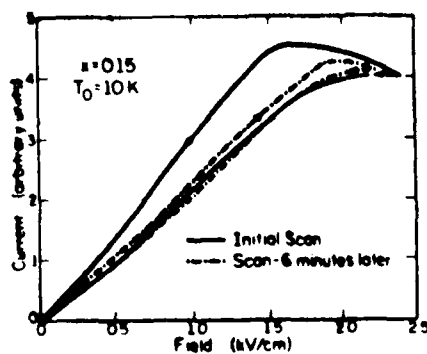
HOT ELECTRONS IN LAYERED SEMICONDUCTORS

THE SIZE OF SEMICONDUCTOR DEVICES HAS DECREASED SO MUCH THAT CLASSICAL TREATMENTS OF SEMICONDUCTOR PHYSICS BECOME INVALID AND EFFECTS INVOLVING SUPRA-THERMAL ELECTRONS TAKE ON A NEW IMPORTANCE.

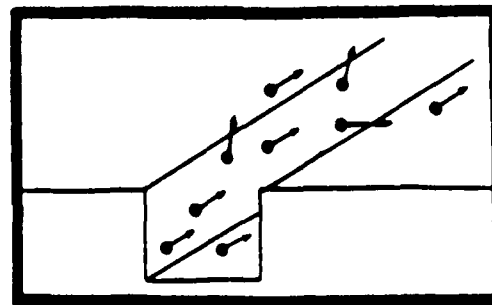
KARL HESS

**The Fifth
International Conference
on
Hot Carriers
in
Semiconductors**

REAL SPACE TRANSFER



HESS, MORKOC, SHICHIJO,
STREETMAN, APPL. PHYS.
LETTERS, 35, 469 (1979)



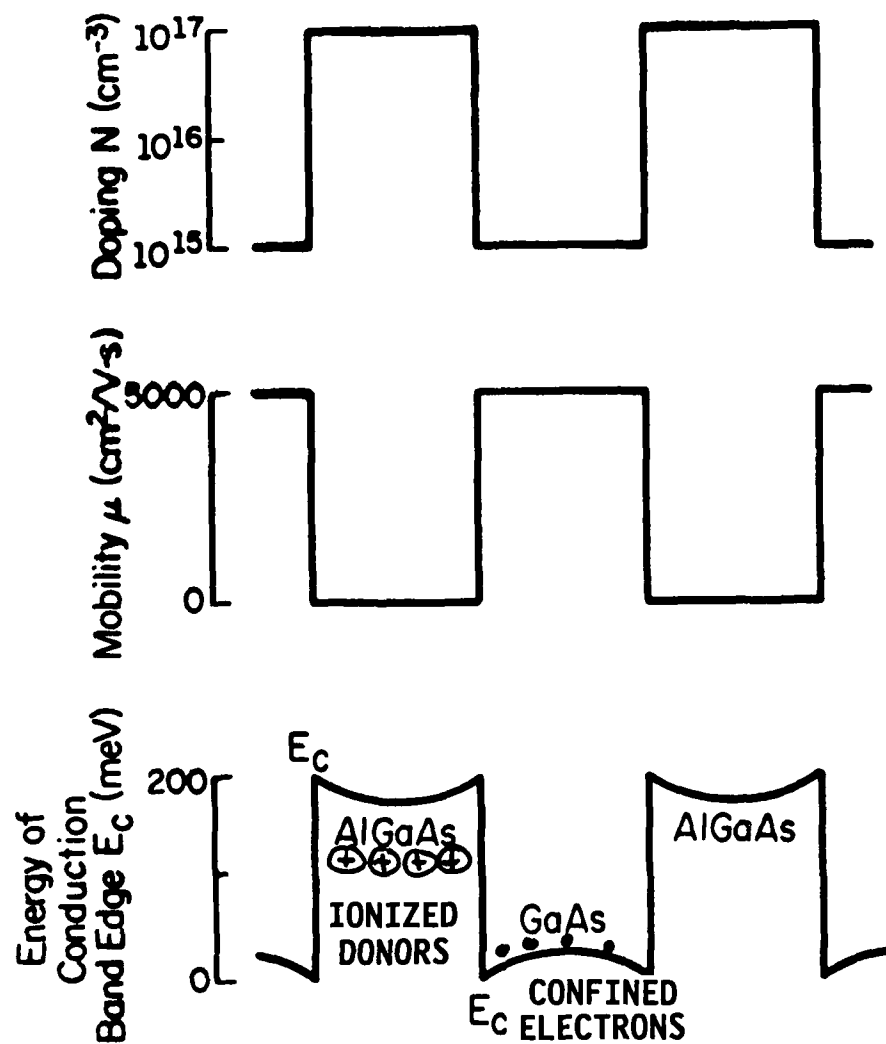
Boston, U.S.A.
July 20-24, 1987
First Announcement

FIGURE 19

REAL SPACE TRANSFER DEVICES

- o DIFFERENTIAL NEGATIVE RESISTANCE
- o FAST SWITCHING (~ 30 GHz)
- o CHARGE STORAGE
- o HIGH TRANSCONDUCTANCE TRANSISTOR

FIGURE 20

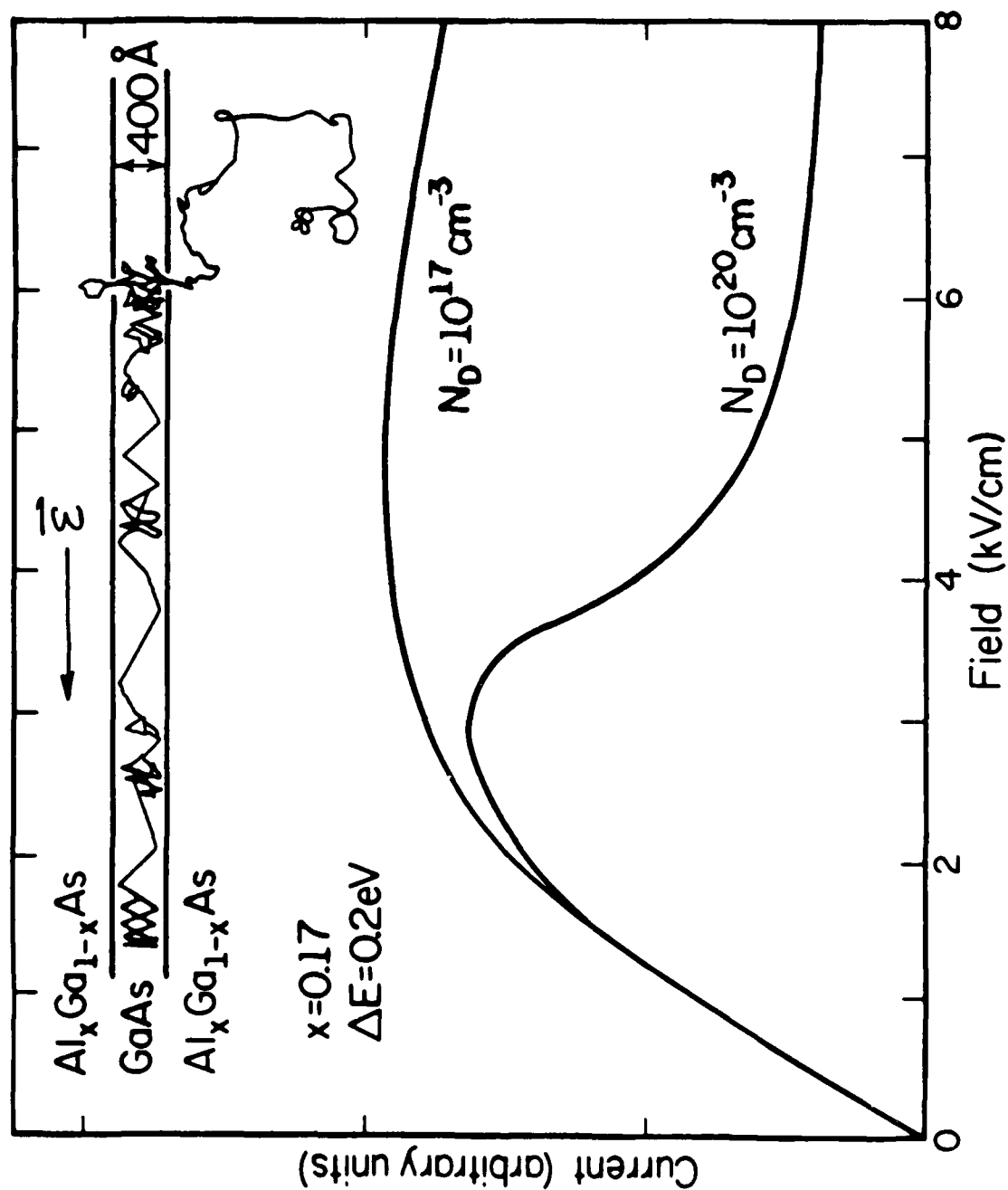


SHICHIJO, HESS, STREETMAN, SOLID STATE ELECTRONICS 23, 817 (1980).

not perpendicular but parallel to the layer. This results in a very high field, high enough to give near ballistic transport of the electrons. Figure 21 shows the Monte Carlo simulation of an electron moving down one of the gallium arsenide layers. If the electron gets enough energy, it actually can scatter into the aluminum gallium arsenide layers, which have at least an order of magnitude lower mobility because of the high doping. The result is a device with negative differential resistance. This effect is very different conceptually and philosophically from a Gunn diode, which operates on a reciprocal space rather than real space transfer. Karl was clever enough to recognize that this basic concept also could be used to produce a high-speed switching device. Figure 22 is from a paper due out in the next two weeks in Applied Physics Letters that predicts switching time in the femtoseconds regime by combining both real space transfer and tunneling effects.

I would like to end by describing one kind of very interesting recent idea from our group that uses electronic ideas to produce property changes you probably have never even considered. Figure 23 shows a transmission electron microscope picture of a superlattice, illustrating the fact that we can grow fifteen angstrom layers, with no real problem. The residence time of molecular nitrogen on a transition metal at the growth temperature is on the order of microseconds to nanoseconds. Without going into any detail, you need all the possible tricks in your bag of tricks, such as highly non-thermodynamic beams and chemistry, even to grow these things, but we have been able to do that. We performed some calculations on the band structure of titanium nitride that told us that for a little over 25-angstrom

FIGURE 21

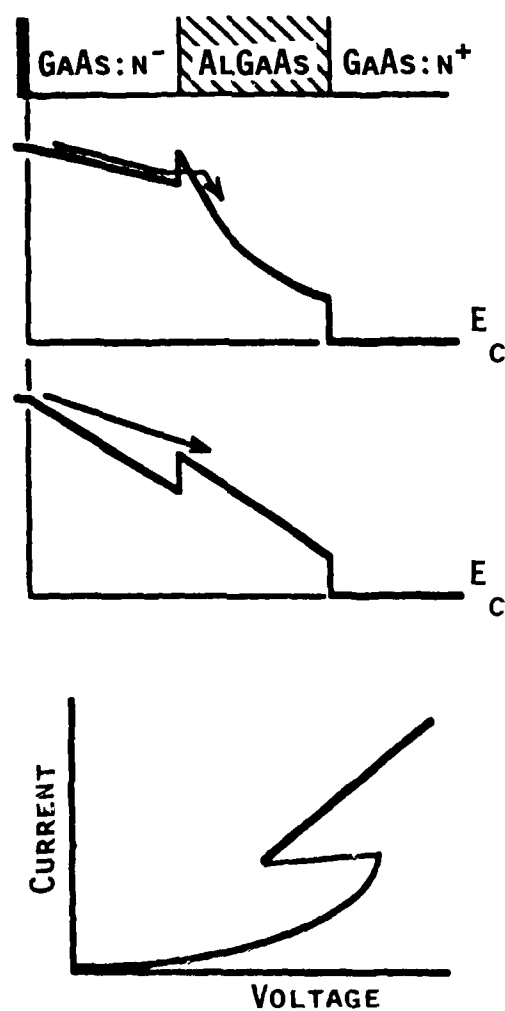


KEEVER, SHICHIJO, HESS, BANERJEE, WITOWSKI, MORKOC, STREETMAN, APL. 38, 36 (1981)

FIGURE 22

NEW ULTRAFAST SWITCHING MECHANISM
IN SEMICONDUCTOR HETEROSTRUCTURES

HESS, HIGMAN, EMANUEL, AND COLEMAN
APPLIED PHYSICS LETTERS, IN PRESS



AD-A187 105

PROCEEDINGS OF THE ANNIVERSARY SYMPOSIUM (48TH) OF THE
JOINT SERVICES ELE. (U) BATTELLE COLUMBUS LABS RESEARCH
TRIANGLE PARK NC D ROBB ET AL. JAN 87 AFOSR-TR-87-1497

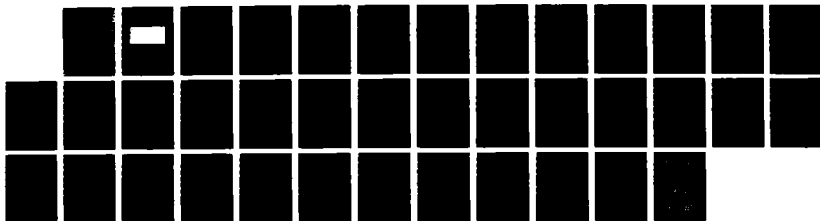
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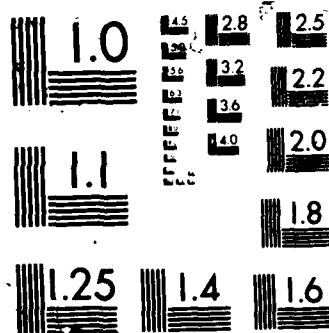
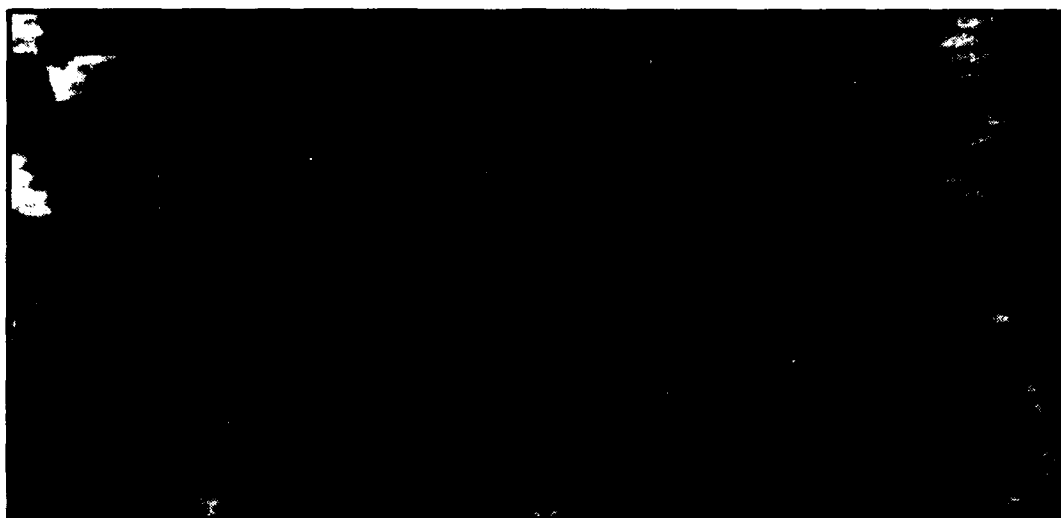


FIGURE 23

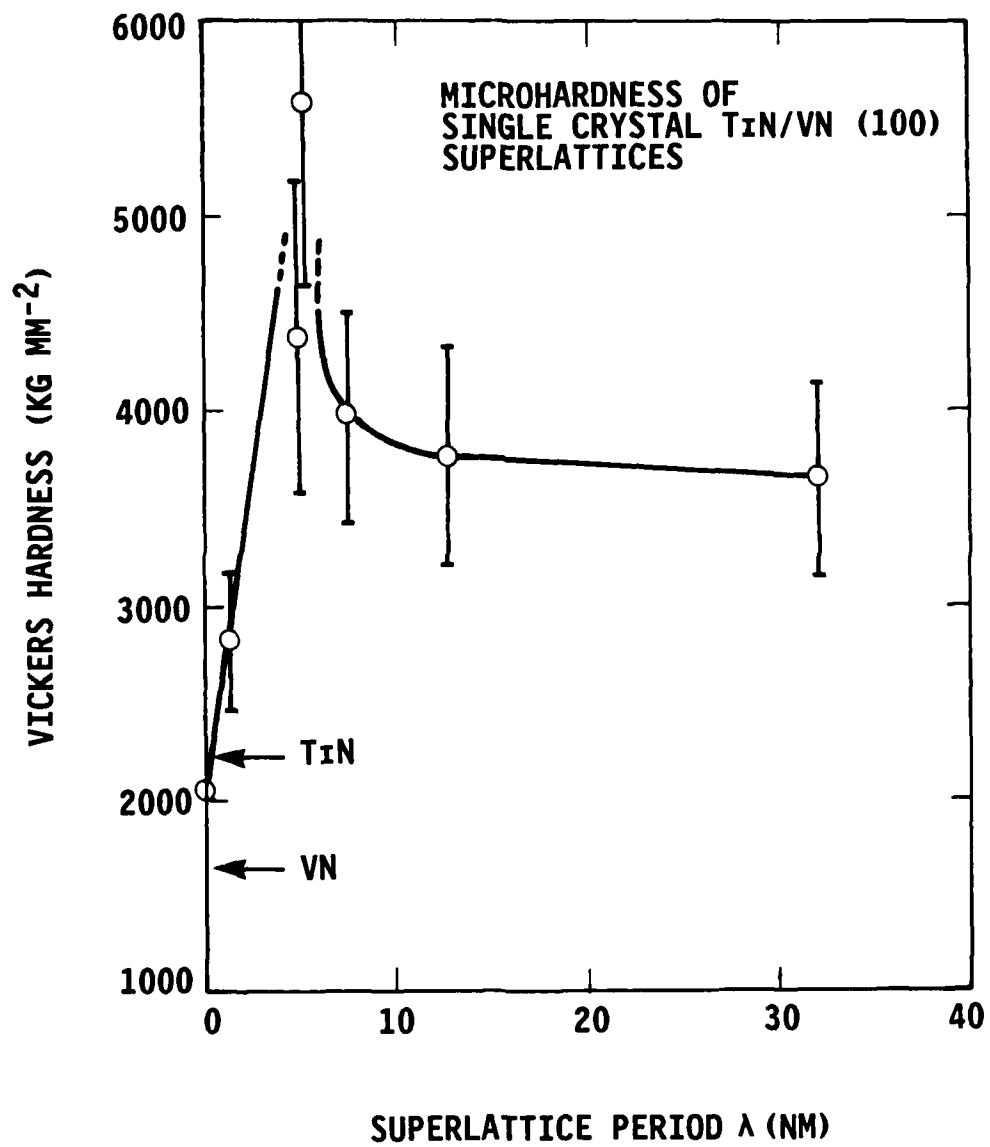
$\text{TiN}/\text{VN}(100)$ SUPERLATTICE
LAYER THICKNESS = 15 ANGSTROMS



HELMERSON, TODOROVA, MARKERT, BARNETT, SUNDGREN, GREENE
APPLIED PHYSICS LETTERS, SUBMITTED

layer thickness we should get an electronic singularity, having to do with the shape of the titanium 3-d and the nitrogen 2-s just crossing at the femisphere. Figure 24 shows the microhardness of the titanium nitride and vanadium nitride superlattices. At the region where we predict the electronics singularity you get a huge increase in the microhardness. In fact, as far as I know, that is the highest hardness that has ever been measured by man on any material aside from diamond. This is a very practical result and I think a very exciting one; using electronics to be able to modulate mechanical properties. Basically all we are doing is changing the potential well by putting in many zones and modulating the elastic modulus. I think this is going to be an important technology for hard coatings or tool bits. Here is an application of electronics to mechanical engineering.

FIGURE 24



PANEL ON PRESENT AND FUTURE JSEP GOALS

Chaired by

Dr. Kenneth L. Davis

Technical Coordinating Committee, JSEP

Panelists:

Dr. Jonathan Allen

Director, Research Laboratory of Electronics
Massachusetts Institute of Technology

Dr. Ronald Kerber

Deputy Under Secretary of Defense
Research and Advanced Technology

Dr. Richard M. Osgood

Co-Director, Columbia Radiation Laboratory
Columbia University

Dr. Timothy Trick

Chairman, Electrical Engineering Department
University of Illinois

PANEL DISCUSSION

Dr. Davis:

During the morning sessions, we heard a lot about the history of JSEP. This included some very important accomplishments that JSEP supported, primarily during the early years. During the first session this afternoon, the speakers described some of the exciting research areas that are currently being emphasized in JSEP.

In this session we will examine the current JSEP program, look at what JSEP evolved into over its 40-year lifetime, and try to look into the future of the program. We will do this by discussing the characteristics that the panelists feel JSEP needs to have in order to remain an important factor in the country's electronics research program. With the tight budgets we are facing, in combination with an expanding electronics research community, we all face a major challenge ahead in the area of basic research in electronics, JSEP included.

I would now like to introduce our four distinguished panelists. Prof. Jonathan Allen is Director of the Research Laboratory of Electronics at the Massachusetts Institute of Technology and also the current JSEP laboratory director at that institution. Dr. Richard Osgood, is Co-Director of the Columbia University Radiation Laboratory, Director of the Columbia Microelectronics Sciences Laboratory, and a current JSEP Laboratory co-director at Columbia University. Dr. Ronald Kerber is the Deputy Under

Secretary of Defense for Research and Advanced Technology, Office of the Secretary of Defense. Dr. Timothy Trick is Chairman of the Department of Electrical Engineering at the University of Illinois and current JSEP lab director at Illinois.

Many of you received a copy of a preliminary agenda in which five panelists were listed. The fifth panelist was Dr. Joseph Pettit, President of Georgia Tech University. I am very sad to announce at this time that Dr. Pettit passed away 10 days ago. He had a long history with the JSEP program. In fact, during World War II he worked at the Harvard Radio Research Laboratory on radar countermeasures, which as we heard this morning was a precursor to the JSEP program. In 1947, he joined the Stanford faculty and worked on the original JSEP program at Stanford. He stayed at Stanford until 1972, becoming Dean of Engineering in 1958. From 1972 until his death he was president of Georgia Tech University, where we currently have a JSEP contract. Dr. Pettit was an outstanding engineer, an outstanding administrator, and for many people in the audience here today, a good friend. He will be sorely missed.

Let me explain how the panel will operate. The Technical Coordinating Committee has prepared four questions relevant to the JSEP program. We have assigned, ahead of time, one question to each of the panelists. After that, additional questions submitted by the audience will be directed to the panelists for consideration. The first question is for Dr. Kerber.

1. JSEP continues to support very basic research with long-term goals. From a DOD point of view what are your expectations for programs of this kind?

Dr. Kerber:

Thank you. It is a pleasure to be here. What the Department of Defense obviously wants is for the nation to remain at the forefront of electronics technology. The kind of efforts in JSEP that I have seen include studies in solid-state materials, quantum electronics, information electronics, and electromagnetics. (I was surprised that electromagnetics was still a part of the program and was glad to hear this.) As you know, technologies are really the key to the defense of the country, since we rely upon a technological edge to offset the Soviet's numerical edge. So, the kind of program you have in JSEP is key to whether or not we will be able to attain that goal and maintain it. I know that Deputy Secretary Taft this morning commented on the need for a strong technological base. I was very glad to hear it, and hope to remind him of that. The tech base is important in many areas, which I am sure you all are aware, but if you look at the Defense Department's program it is strongly skewed toward electronics-type technologies. We are interested, in a major way, in computers and C3, avionics, electronic warfare, detectors, sensors, etc. Electromagnetics plays into the area of stealth. All of the technologies that are involved in the JSEP program are critical to maintaining this technological edge.

There are a couple of other things I should mention relative to how we view technology. I think there was a time when technology was viewed as something that provided capability. Now we are not only looking for it to provide capability but also affordability. I am reminded of a staffer who came over from the Hill and said, "The problem with the tech base is that everytime we put a dollar into it you dream up ten dollars worth of new things for systems." That is a misconception we have to address. The key thing is that we have many programs which are using technology to make things more affordable, more efficient, and more producible. Basic research not only gives us capability but it also gives us affordability.

The JSEP program is obviously a producer. We have seen many things coming from the program over the years in an amazing stream. In particular, the program has lasted long enough so that it is possible to see distinct technologies originating in JSEP and ending up in the field. A program that lasts 40 years certainly provides such an opportunity. In fact, we need to take advantage of those opportunities to make sure that all parts of the defense establishment realize the key role of basic research. In addition to basic research and the ideas that end up as part of our operating systems, we also get trained scientists and engineers who are keyed to building those systems once we have the concepts and ideas. Also we are very proud that some of the leading researchers in the world have been part of JSEP and have won distinguished prizes and honors. It is a very impressive group that has been supported by JSEP over the years.

Let me make a couple comments about block programs and what we see that they can do for us and what we see as a role for other kinds of support in the basic area. The block program, of which JSEP is an example, obviously allows one to plan for a period of time, so that resources may be committed by the universities. This includes commitment of faculty and students and provides a kind of stability that is important in developing strong university systems. I think it also allows some risk taking. With short-term grants or performance-oriented grants, it is a little more difficult to take risks because near-term productivity is expected in order to be renewed. I think the greater opportunity for risk taking is a positive aspect of a program like JSEP. Another positive aspect is the continuity of interaction with the Department of Defense. I think it is important that the department have its own scientists and engineers linked with some of the strongest scientific people in the country, to help the DOD grow technologically, and to ensure their projects reflect the kind of thought process that goes on in the universities and in programs such as JSEP. So I think there are a lot of things that come out of a program like this, such as developing these personal ties and developing risk-taking planning ability. Of course, I think you are aware that we have also developed other block programs but we do not at this time anticipate another program that is likely to last 40 years. We have the University Research Initiative (URI) program and we have had instrumentation programs with a similar flavor. URI is related to JSEP in that it is also block funding, while the instrumentation program was intended to solve a specific need in the universities.

Well, what are the expectations for JSEP? JSEP has, in my view, delivered all of the above features. In addition, I have been surprised to find that this program, with leadership that seems somewhat established in certain technology areas, has been able to track technological opportunity. JSEP has clearly done all that. It is an indication that neither the program nor its participants are stagnant. They study significant technical opportunities and the program moves toward those opportunities. Over its history, JSEP has followed the greatest opportunities in technology and these technologies have been very successful. This is probably because we have had some of the brightest people in the country working in the program. Another thing that seems to be clearly good about JSEP is that from a university perspective there are several ways to take advantage of the program. For example, it can be used for seed funds to begin research for individuals or it can be used as a recruiting tool. There are many JSEP features which can be used in a university environment to give continuity and strength to the university research programs.

We also not only feel that block grants are important, but we feel that the individual grants based on merit and the need of the department are important. That is really the mainstay of our research. We have seen that many of the people who start on the JSEP program end up being supported by the normal competitive process which we have in our offices of scientific research. That is exactly what we like to see. I assure you that the department feels that a strong competitive scientific community, in which awards are based on technical merit and the needs of the department, is really the critical ingredient for making

sure that we have the strongest possible technological infrastructure in the country. It is certainly our goal to maintain that status. I am sure you are aware of many of the problems we have had relative to maintaining that kind of approach to supporting technology. I would certainly ask that everybody in this room help in keeping that a goal of the country's tech base. In closing, I would say that from what I have seen of JSEP, the expectations are high that it will be as successful in the next 40 years as it has been in the last 40 years.

2. JSEP traditionally has been tied to the research needs of DOD. In recent years, industry's role in university research has increased. What should be the appropriate balance between industry objectives and DOD objectives?

Dr. Osgood:

I move to the microphone as a JSEP "grandson." I was surprised to learn, only after I got to Columbia, that my intellectual father was actually a "son" of JSEP. I am speaking of Prof. Javan, of MIT, who in fact was in the JSEP program at Columbia when he was a student. The tradition continues. My own son (my real son, not my intellectual son) is at MIT right now working on lasers in the JSEP building there.

One point I would like to make before answering the question is that, as you know, one of the major research emphases right now for JSEP is in solid-state electronics. In that field, there is a natural involvement between industry and JSEP. It is not something that must be forced, but rather something that will grow naturally. In that regard, we are competing with Japan in this field. It is very important to emphasize that research on solid-state electronics is very, very important in so far as our industrial, and ultimately defense, preparedness is concerned.

Now I would like to answer the question. We all have heard a lot about industry lately. It has been discussed relative to a number of block funded programs, but how much real participation by industry is there in universities? I discovered by looking at an article in the New York Times that it is approximately 15 percent. That is not an awful lot--nowhere near as much as that contributed by the federal government. I presume that the 15 percent is an average number, and that in particular areas of research we might find the number somewhat higher. Another point is that there are other block funded programs, each one of which has its own emphasis on industry. For example, at the Columbia NSF Engineering Research Center in Telecommunications, the involvement with industry is paramount. That feature is built into the system almost from the very beginning. We are supposed to work with industry and actually have them contribute to the program. In the University Research Initiative, industrial interaction is encouraged but not demanded. The interest in that program really involves interchange directly with the government itself. There are other types of centers that may be mentioned. In the

National Science Foundation program there are various centers, for example, the Spectroscopy Center at MIT (with which I have had affiliations in the past). At that center, industry collaboration is really not brought up at all, at least not until very recently. On the other hand, in the IBM Materials Science Centers (one of which is at Columbia) industry involvement is actually written into the basic contractual documents. So there is a whole span of involvement with industry, government, and other universities as well.

Interchange between industry and universities is two-way, as illustrated by the example Prof. Townes gave this morning, in which he described work on elements of the laser and the maser in collaboration with people at Bell Laboratories. On one hand universities can contribute to industry by providing qualified research people. Columbia, for instance, has furnished many people to IBM, Bell Laboratories, and many other companies. In addition to people, of course, the universities also contribute revolutionary ideas. There is nothing like a university where a young native graduate student can propose something crazy and to have that crazy thing hit an old professor who is hungry for a new idea. They can pick it up and run with it.

I think the universities also can do for industry something that is not normally discussed--they can contribute deep studies. By that I do not mean moldy old studies. I mean that they can really afford to look into a phenomenon carefully and in detail. A good example of this was mentioned by Prof. Greene today. Many people in industry are growing MBE materials, but few are actually looking at the

kinetic processes involved. It is only when one looks at the kinetics of the MBE process that one can really start to develop vital insights into growing new types of materials. In other words, one can do a lot with empiricism, but it is only after taking a second look at the problem that clever new insights emerge.

What can industry contribute to the universities? First of all, it can contribute scientific techniques. As an example, we currently are trying to do some spectroscopy on silicon at Columbia. In that connection we have talked to many people in industrial laboratories with the result that we have gotten many good ideas on how to do that work. That is an example of an industrial lab actually doing something first, and the university people learning from industry. Another important issue is visiting scientists. That is something which is quite important in the NSF ERC programs. Visiting scientists are written into the contract. In this way industry can contribute people who are experienced and somewhat more mature than the average graduate student. In addition, industry can contribute funding--everybody wants money, so that is an easy one to talk about. One valuable method is equipment contributions. Another is support for students. I think industries really know that universities are the source of students, so they tend to be generous in contributing fellowships.

Since both universities and industry have a lot to offer each other, what we have is a real partnership between industry and the university. However, one is always cautious about the problems you can have. We have certainly seen a few problems at Columbia. For one thing there is a

big concern with patents. The minute you start to talk to some industries, and I must say the universities are not guiltless in this either, there is always talk of patents, patents, patents. And of course patents never materialize or do not seem to ever materialize, but everybody is worried about them. So, that can be an impediment. Another problem is that direct industry funding to a university faculty member for research typically has a relatively short focus. Industries are naturally concerned about the bottom line. They want to give you money, but they want to see some particular area of research done. I think that can be very useful to a university person, but only if it is the right time in your research to do something specific. If not, then it is deleterious. Another important point is that the amount of industrial money for contracts is typically rather low, and the amount of paperwork or administrative work is high in comparison to JSEP. In spite of the problems, it is a great pleasure when industries contribute interest--there is nothing like encountering somebody in industry who really wants to use your research results for something real. That is a really exciting thing. I like to see something turn out to be practical in the end.

Finally, let me point out that there are two common elements between industry and JSEP. The first is that industry is typically interdisciplinary, i.e., it is typically concerned about something that crosses disciplines. They do not care whether it is defined as chemistry or electronics or whatever. Industry has that attitude with JSEP. The second element is the interest in electronics that many industries have in common with JSEP.

So in the end, what do I think about our collaboration with industry? What do I think the appropriate balance should be? Within JSEP, DOD funding supports novel and future research areas. I simply do not see industry supporting that. For example, if you are going to do research in an unusual semiconductor system, you will be hard pressed to get industry to support it. I think industries recognize afterwards that basic research was useful but they do not perceive it or cannot afford to perceive it at the beginning. So I think you do not want to consider industry as being the main contributor to a JSEP program. Industry would change the character in the wrong way. However, I do think industry can augment a JSEP program. Last year at Columbia, for example, we got a post doc from IBM who has been very helpful to us in setting up a surface analysis system. This is a good example of how industry augmented a program for which we already had money from JSEP. In a similar vein, if you brought a JSEP research project up to the point where it was starting to become practical, industry money contributed at that step could be quite helpful in achieving potential payoff.

3. In addition to longevity of funding, JSEP has a unique style of management involving tri-Service cooperation and DOD laboratory participation. How has the style of the administration of JSEP influenced university productivity and research focus?

Dr. Trick:

I am not sure to whom I should credit this question, Dr. Suttle, Dr. Wittmann, or Dr. Davis. In any case they sure enjoy throwing JSEP directors into the frying pan. When I read this question I thought that I needed guidance and I recalled a story told by Dean William Everett on getting guidance. Dean Everett was one of the giants in our profession. His 1932 textbook on communication engineering gave birth to a new field of study in electrical engineering. Sadly, I report that Dean Everett passed away on September 6, 1986, at the age of 86. So we have lost two giants in our profession this month (Joseph Pettit was the other). Dean Everett's parents died when he was young boy, so he was raised by his Uncle Ben, a preacher. When he first became Dean of Engineering at the University of Illinois he sought Uncle Ben's guidance in the awesome responsibility that he was about to undertake. Uncle Ben said, "The next time it rains, go outside, look up to the heavens, and guidance will come." Well, Dean Everett tried his advice and the next time he saw his Uncle Ben he said, "I went out into the rain, looked up, the rain ran down my face and the back of my neck and I felt like a damned fool." Uncle Ben replied, "Wasn't that quite a revelation for the first try?" Well, here I am, feeling like a damned fool trying to answer this question.

I think there are probably four words, at least in my mind, that describe JSEP. These are longevity, continuity, communication, and flexibility. I will say a little bit about each of these words, and try to bring forth some examples from my own experience that I think will stick

with you and answer the question that was put to me. First I would like to focus on the word longevity. I suspect that if I asked for a show of hands, we would all agree that longevity is crucial to the success of good research. Yet all of us are guilty of paying lip service to the importance of longevity and continuity of research funding, and then acting to the contrary. You have probably heard that Arnold and Mabel Beckman donated \$40 million to the University of Illinois toward the creation of a \$50 million Institute for Advanced Technology. I serve on a committee that is establishing the administrative policy for this facility. A few misguided souls on this committee wanted to be fair to all the faculty, and proposed a revolving door policy in which faculty would serve 3-year terms in the institute and then return to their academic departments. Fortunately, the committee sought the wisdom of a number of other research laboratory directors on campus and sanity prevailed. Director after director reported that the time constant in academia for good research is at least 10 to 12 years, and that a 3-year revolving door policy would be very disruptive to good research. Usually 4 to 6 years are needed to nurture a young Ph.D in a stimulating environment in which kindly advice is available from one or more sages. Then one can expect 10 to 20 productive years before administrative burdens get the upper hand. JSEP has had longevity and continuity and has kept administrative burdens to a minimum.

I am amazed at the number of outstanding researchers who have benefited early in their careers from JSEP. For example, our Chancellor, Tom Everhart, was in the JSEP program at Berkeley. He asked me to extend his regards to all of you and he is sorry he could not make it here, but he

wanted me to express how appreciative he was of JSEP support early in his career. And of course Dean Van Valkenburg, who is with us today, Dean of Engineering at the University of Illinois. People like Don Bitzer, an inventor of the plasma display panel. People like Seshu, who perhaps only some of you old timers remember because of his early and untimely death. He did a lot of exciting and interesting work in fault diagnosis of analog and digital systems. I still see some of his early papers referenced today. People like Ben Streetman, who is now at Texas, and of course young people who are coming along like Joe Greene who you heard today, and others.

JSEP has been successful because it has had continuity, longevity, flexibility, and feedback or communication, whichever word you want to use. The management style of JSEP has been such that it has allowed for the creation of an ideal environment in which young faculty can be nurtured. At the University of Illinois, JSEP-sponsored projects typically include at least one senior faculty member, and usually a junior faculty member. The senior investigator is expected to have other research support and is expected to play the role of the sage. This promotes the longevity and continuity. Another unique feature of JSEP is the feedback obtained in periodic reviews in which scientists and engineers from DOD laboratories and offices of scientific research have an opportunity to meet, one on one. These reviews give DOD personnel an opportunity to learn more about the capabilities of the university and to critique the research. Faculty have the opportunity to meet their counterparts in DOD and to learn firsthand about their concerns. These meetings are much more effective, in

my estimation, than telephone calls or an isolated visit to a DOD staff person. In fact, we have found this forum to be so successful in exchange of ideas that we now periodically stage reviews for industry. These types of exchanges are very effective in influencing the research focus, and are second in importance only to money. For example, Karl Hess told me that he would probably still be working with silicon if he had not been introduced to some of the DOD people in the JSEP program. There are a number of other examples I can cite which have been very, very effective in changing the direction of focus.

Finally, the JSEP management style allows the director flexibility--the value of this characteristic cannot be underestimated. Typically one year is required from the time a proposal is prepared to the time funding is received. One needs to move much more quickly when research opportunities present themselves. JSEP gives the program director the flexibility that is needed. Furthermore, this flexibility is enhanced at the University of Illinois by the fact that the university gives the director control over 25 percent of indirect cost funds. These funds can be further leveraged by funds from other university sources, such as the College or the Research Board.

Let me illustrate how important flexibility is by an example that I am sure many previous directors at Illinois could cite. Recently many of our faculty put together a plan for a \$6 million Molecular Beam Epitaxy facility. It was put together by faculty from material science, chemistry, physics, and electrical engineering. At a 50 percent manufacturer's discount, which they were able to get, we

needed about \$3 million--they had raised about \$2 million. They came to me and asked, "How are we going to get an additional \$1 million?" By having flexibility in the JSEP program we were able to commit \$250,000 from the JSEP program, \$250,000 from our indirect cost fund over a 3-year period, and I was able to go to the university and say, "I really believe in what these people are trying to do, and I put up half a million dollars, won't you?" They did. This is but one example of how JSEP directors with the confidence of the DOD TCC can move quickly to fund exciting new research opportunities. Getting the necessary tools to do good research in a timely way means better productivity.

As a JSEP director I learned how to take risks in hopes of enormous payoffs down the road. Hopefully I took intelligent risks, as did my predecessors. In any event I learned to be an optimist. As Dean Everett used to say, "I am an optimist rather than a pessimist. It is possible that the pessimist may be proven right in the long run but we optimists have a better time on the trip." I also gained an education and experience as a JSEP director. In case you do not know the difference, let me give you a lawyer's definition. Education is what you get when you read the fine print and experience is what you get when you don't.

4. The influence and relative importance of JSEP is affected by competing centers and by inflation. What steps should be taken to insure the continued viability and uniqueness of JSEP?

Dr. Allen:

That question is really quite apropos for me since RLE at MIT is a 40-year-old laboratory, in addition to being in JSEP for 40 years. Let me deal, first, with the part of the question that deals with the problem of competing centers. Currently, there is a tendency to invent new centers. In fact this trend seems to be accelerating recently. Not only are new centers being created, but, in fact, a number of programs are coming forth now that demand new centers and, in addition, demand whole new administrative structures. I do not think that is a terribly good idea. In addition, as laboratories grow older, they get larger, as Prof. Henry Zimmerman documented this morning. In the case of RLE, they got bigger and bigger until someone (in this case it was Gordon Brown) said, "Maybe we should have some other laboratories." I tend to think it is rather like a biological process, sort of like mitosis; a cell gets bigger and bigger and bigger, and then there is a split off. We have seen quite a bit of that at MIT. As a matter of fact, splitting off from RLE has happened over the years in a number of cases. For example, a lot of early computing work that went on at MIT was pursued jointly between the Computing Center and RLE, but there came a time when there was particular emphasis on time-sharing. As a result, people wanted to split off and form what was then called Project Mac, which was later renamed the Laboratory for Computer Science. The process did not stop there. The Artificial Intelligence Laboratory, which was initially part of Project Mac, split off from it. And so this process keeps going. We have had other examples. Prof. Zimmerman mentioned this morning that the National

Magnet Laboratory split off from Francis Bitter's work. We saw in 1980 the Plasma Fusion Center splitting off. This was a very large laboratory designed initially for mission-directed purposes, such as building very large confinement machines. In a way, this activity was inappropriate for basic fundamental research within RLE. In addition, we have seen a lot of our natural language work split off to form a new Cognitive Science Center. The splitting off, then, is something of a natural process.

Now we could accept that, I suppose, but on the other hand, when a new center is formed my experience is that it cannot be invented out of thin air. That is, you do not bring in a whole new group of faculty and a whole new staff. Instead these new centers get their body cloth, if you like, and social fabric from the existing laboratories. That means, a little more bluntly, their emergence comes out of our hide. So, the laboratory needs to concern itself with maintaining its overall structure and viability in the face of such changes. Further, there is an ongoing tension between, on the one hand, diversity (and as the lab gets bigger you get more diversity; certainly we have that within RLE) and on the other, necessary coherence in the laboratory program. Sometimes when new centers are formed, it is an attempt to find more identity and coherence within a new group that may, at least initially, be more tightly focused than was the parent body. I think these tensions are, to a certain extent, inevitable. In a large laboratory we have to try to walk the line between this coherence and a diversity of focus. There is no question in my mind that if we have to decide one side or the other it has to be in the name of diversity, because that is where

fundamental interdisciplinary research is done. After all, that is what a laboratory like RLE was invented to do--to bring together physicists and electrical engineers to do interdisciplinary work. Now that also means we have to try to find (and I think some of these new groups are trying to find) what you might call new natural joints in the intellectual structure of various problem areas. On the other hand, my experience is that in most complex problems there are a number of different principal areas with which the investigation is concerned (physics, electrical engineering, and physical chemistry for example), but that these also have a substantial and substantive overlap among them. That is why the laboratory is there. Therefore, while these new centers are growing in various ways, I think it is also very important to keep a broad, fundamental strength. That is what I see going on in JSEP in a way that spans these often conventional disciplines. So I think the answer to what should JSEP do about the creation of new centers is that it must maintain its fundamental breadth. It cannot deviate from that because that is the heart of interdisciplinary research. On the other hand, it can, and in MIT's case RLE certainly does, build very strong links to these new and emerging centers. They will surely continue to blossom. In fact, my current boss, Ken Smith, the Associate Provost at MIT, sometimes calls RLE the interlaboratory laboratory, because it has become sort of a mother node, if you like, for many of these other laboratories. I think the key idea is to maintain this broad diversity, but try to build focus within and build the right links in order to attack problems that need broad interdisciplinary research approaches.

The second part of the question had to do with inflation. As I thought about this question I realized that there was more than one kind of inflation that we really ought to consider. The most obvious one, of course, is the one we face with dollars. That is a very real one. Some years it is worse than others, but it is one that really has to be dealt with. The main comment that I would put forth is that if funding becomes flat (in terms of contract dollars as opposed to normalized dollars) or begins to slack off, then it is very hard to pull back and regain momentum after that time. So I feel that for an ongoing and viable program which has the kind of characteristics that Tim Trick just discussed, it is very important to be able to at least track the dollar inflation. Unfortunately, I believe that there are other kinds of inflation that are also going on, and we ought to mention those. First of all we are not doing the same thing that we were doing 20 years ago. We just heard about buying an MBE machine for a very large sum of money, and the attendant problems of raising funds for that machine. The cost of doing our research is going up a lot. You might call that another kind inflation, if you like. These pieces of equipment that we need are very expensive. Many of them need fulltime, often post doctoral staffs, to go along with them, as well as technicians. So there is that escalation, too. In my opinion, it is a very large escalation. It is not as if we just need to keep going in terms of dollars, we must also realize that there is a qualitative change in the nature of the enterprise in most JSEP programs. That is certainly true at MIT and I believe it is also true at the other schools.

Finally, there is one other kind of inflation that I would like to mention. It was alluded to earlier in Rick Osgood's discussion of JSEP's interrelationship with industry. That is, we are increasingly competing with industry for personnel. It is not uncommon for us, when we are recruiting faculty members, to find that their decision of whether to choose an academic career as contrasted with working in a good industrial research laboratory is a hard choice to make. Since we are having to deal with that particular problem, it means that the cost of attracting people is going up. Salaries are increasing and I think universities are realizing that they cannot be apart from the competitive forces going on in industry with respect to salaries for professors and staff people. So I tend to see the question of inflation as having these multidimensional characteristics. Not just dollars. Dollars are certainly very important, but the situation is exacerbated further by these additional characteristics which we need to deal with. That poses additional problems for large block funded programs of this sort and certainly makes their operation increasingly difficult.

Additional questions from the audience:

Since JSEP is characterized by long-term funding continuity, isn't JSEP a "good old boys" group of a few selected participants?

Dr. Allen:

We have heard that question many times before, believe me, and I would say two things. One is that if you just sit down and look at the facts, you will find a frequent turnover of personnel within JSEP programs. Even more important, JSEP is one of the very few programs that gives a laboratory director discretion to provide new seed money for new faculty members. Rather than being just an old boys network, I can say that when new faculty members come into our laboratory, and we feel they are appropriate to JSEP, then they come into that program. So in fact, instead of accusing JSEP of being an old boys club, it is quite the opposite. It provides the mechanism to bring in new blood.

Dr. Osgood:

I think you also can have old boys in terms of the university being an old boy. I am not an expert in the history of what JSEP has done, but as I understand it there have been important changes in the actual membership of universities in JSEP. Just speaking from our experience at Columbia, I must say that at one time we had some problems with graying hair. We had to reorganize or we would have become an ex-old boy. There is certainly administrative pressure put on the member universities to keep current and to maintain good programs. I understand, too, that there is continual encouragement to get new member universities into the program as well.

Dr. Davis:

I think I should amplify on that a bit. In the last 7 years, 2 JSEP universities have ceased to be JSEP universities and 5 new ones have joined. There is quite a bit of dynamics. JSEP is actually advertised in Commerce Business Daily. Any university in the country has a right to write a proposal and go through exactly the same review process that all the JSEP schools must go through. The program is open from that point of view.

Dr. Trick:

Let me amplify the topic of nurturing young people. I think as I look back that there is hardly anyone from the 1960s who is still in our JSEP program. They are almost all new people. A lot of these new people have been brought in by the old and nurtured into that period of maximum productivity. Typically, we see time constants of 10 to 15 years of active research, with new blood constantly being brought in. There is a constant turnover.

How can you respond as a laboratory director to changing topical areas of emphasis?

Dr. Osgood:

One way that has already been talked about is that as you see new people coming in, you can selectively encourage

these people to get into JSEP. For example, at Columbia we have been very interested in bringing solid-state people or people connected with at least the physics of electronic materials processing. That has been a big emphasis with us. Over the last several years, we have brought in three people, including myself, who were seeded in the JSEP program. So our program has become a lot more heavily oriented in solid-state research. We were thus able to respond in a measured way to an interest in emphasizing a particular area of solid-state electronics.

Dr. Allen:

Let me just add to that by saying that I see this question as related to the earlier question. There is a natural evolution of interest in which topics build up while others wane. Part of the laboratory director's job is to provide leadership in those new directions. It involves some people dropping out of the program and others coming in. It is part of the dynamics of an ongoing and healthy JSEP program. I see it as the way we normally do business.

Dr. Trick:

I will just second again what Jonathan had to say because as a JSEP director it is your responsibility to listen very carefully to DOD and the TCC, and to see what their problems are as well as their areas of research interest. I know I go back and talk with my faculty about the DOD problems. I listen to their responses. I have to

make some very hard decisions. We had a 3-year review recently, and I had to turn down a number of proposals. It is not fun to deny people the opportunity to be part of the program, but it must be done.

Dr. Allen:

Let me just add one other thing to that. At MIT, and I am sure this is true at other JSEP schools, the program is of such high quality that people want to get in like mad. There is almost a waiting list of people who want to be part of JSEP. They understand that nothing short of the best quality is requisite in that program. We can deal with these people since they understand what the requirements are.

The government, through many agencies, is currently sponsoring research centers to support focused areas of research. These centers are block-funded for a specified time period. By contrast, JSEP has had continuity of funding for 40 years. What are the relative merits of these types of university support?

Dr. Kerber:

We do have a new program, as you know, called the University Research Initiative. When I took this position I was quite sensitive about the concept of small but focused programs. The goal of the URI was to fund just the people

that we thought were the top people, and to bring together interdisciplinary groups to work on focused problems. The duration of those, given a healthy bill from Congress, would be 3 to 5 years. We see those centers as clearly different from JSEP, which has lasted so long, although there is no reason to believe that through the normal funding process those activities would not continue to be funded through individual grants in the years to come. In fact, we would expect that. The goals of the URI efforts were to bring together the interdisciplinary teams of just the top people. I am very sensitive to Dr. Allen's concept that establishing a center provides with it some overhead. It was our hope that this would not happen with these activities, but that we would only have a small nucleus of scientists working together, rather than a whole administrative structure to support them.

Dr. Trick:

I think these short-range programs are a real trauma for the university. They do bring in a lot of money, and you may be ramped up with a million dollars a year for 3 or 4 years, with many thesis students in the pipeline, when, all of a sudden, it truncates. Also, if you look at what it takes to develop young research people, nurture them, and make them productive in research, these programs really do not answer that need. The university should be developing a good scientific and engineering base of well-trained people. This takes years to develop, to the point where DOD can call upon them when it has focused problems to work on. If we are going to have these big programs, I wish

somehow they could ramp up and down more slowly. That would be enormously helpful.

Dr. Allen:

I would like to agree with what Tim says. I think there is a tendency sometimes to come on hard with a lot of money, which subsequently drops out. In some of the short-range programs, there exists the implicit assumption that perhaps we can figure out how to deliver a baby in less than 9 months by putting more men on the job. There is the feeling that we can get around the natural gestation period of science by increasing intensity of manpower and funds. As Tim pointed out, it takes a long while to build up a program and to get it moving. We need a longer time constant.

Dr. Osgood:

I think one should look at what the program really is, what it is really going to do, and then evaluate it. Nobody is going to turn down a 1-year, \$2-million instrumentation program. On the other hand, if one has a 5-year program of large magnitude in which after one year it is not clear that it is going to last 4 or 2 or zero years, that is terrible. If DOD or Congress cannot guarantee continued funding for the program, it gets ramped up and down, resulting in a real mess. The real problem is the uncertainty and instability in the funding mechanism.

Dr. Davis:

The final question is for Dr. Kerber.

Deputy Secretary Taft expressed pride regarding DOD's accomplishment in increasing the total funding for the R&D tech base. Other speakers mentioned the importance of core fundamental research, of which JSEP is a part. What can be done to enhance the support for core basic research funding in DOD?

Dr. Kerber:

I think the real key involves the problem that has been alluded to already. The Defense Department, in general, needs to develop an appreciation for the fact that the tech base exists to provide us with operational capability. It takes time to do that and important technologies have emerged from the tech base. Basically, the technology base of the country takes a long time to establish and requires funding to maintain it. It turns out that not only in the Pentagon but on this side of the river too, everybody is impatient. As you probably know, the URI program went to Congress at \$25 million and came out at \$100 million with a lot of enthusiasm. This enthusiasm diminished with time. The key to stability of support by the department (a job that of course is mine) is to educate the systems planners, the strategists, and the policy makers, in the role of the tech base and the fact that they have to be patient. The fact is they need that base if they are ever going to be able to draw upon it. Some activities are short term and

some are long term, but I think in general the best thing that can happen to the tech base is continual discussions that make everybody aware of what it does for them. We hear the leaders of the country say that its military strength is built on our technological edge. The tech base is the foundation of that technological edge. It turns out that people constantly need to be reminded of that. As you look at DOD priorities and the budget cycle we are going through now, first of all you need to protect the military people--that is a big issue. You also need to buy some guns and bullets, and then you need to build some new systems. Finally, you need the tech base to allow you to do all that in the future. That is the scenario we go through. We must remind everybody that the last thing we mentioned (R&D) is what all the rest of the DOD priorities are built upon. So I think it is really a continual education process. It is one that has been going on forever and must be continued.

Dr. Davis:

Our time is up for the panel discussions. Thank you all for your interest and participation.

CLOSING REMARKS

Dr. Leo Young

It is time to conclude. We have listened to the remarkable history and accomplishments of JSEP over the past 40 years, and we expect equally great things in the next 40. I have been asked to summarize what was said by our distinguished speakers, and have reduced it conveniently to 10 points as follows.

(1) The distinction between BASIC and DIRECTED research was made by Charles Townes, who pointed out that only the former could have produced the laser, while the latter is more appropriate to improve a light bulb. If the goal of JSEP is to be more lasers rather than better light bulbs, then the preponderance of its research should remain basic.

(2) The core support provided by JSEP results in FLEXIBILITY, a point made by Nicholas Bloembergen, Arthur Oliner, and John Whinnery. It encourages team effort and long-term commitment.

(3) The stability and longevity of JSEP programs allow RISK TAKING, as noted by Deputy Under Secretary Ronald Kerber. This is often a prerequisite before a major advance.

(4) JSEP is well suited to provide SEED FUNDS for following up quickly on new research results or ideas.

(5) The importance and influence of EQUIPMENT on quality research is growing. JSEP continues to be helpful in its acquisition, as suggested by Henry Zimmerman and John Whinnery.

(6) They also noted the importance of good COMMUNICATION among research workers. JSEP creates the right environment.

(7) INTERACTIONS must be good between the universities and the Services, as Arnold Shostak pointed out, if great results are to be achieved. JSEP has built up that kind of relationship.

(8) There also are many healthy connections with industry, which enables JSEP to train graduates to become LEADERS.

(9) JSEP has survived so long--and indeed is serving as a model for similar new programs--because of the many RESEARCH RESULTS and NEW IDEAS it has contributed over the years. The names of the speakers on today's program bear witness to that.

(10) Last but not least, let me return to the theme sounded by our keynote speaker, Deputy Secretary of Defense William Taft, which is PARTNERSHIP. Only by extending the practice of partnership and cooperation as in JSEP are we going to get the most for our research efforts. The measure of success will then be world peace and security.

I would like to express my personal thanks to all those who have made this meeting possible, to Arnold Shostak, Jimmy Suttle, Ken Davis, and Horst Wittmann, as well as to John Dimmock with whom I worked closely when he managed the Navy's JSEP program. I hope to see you all again in 10 years' time, at the 50th birthday of JSEP.

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